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HABITUATION TO ROTATION[1]

BY RAYMOND DODGE

Wesleyan University

Does the human organism become adapted to rotation? The question has several important bearings, practical as well as theoretical. During the war, defence of the rotation tests for aviators took the form of assumptions that outran knowledge in more than one particular. One of these was the assumption that after-nystagmus could not be modified by habituation. To some of us this assumption seems quite irrelevant to the value of the rotation test. But the question, whether the objective and subjective effects of rotation are

[1]The present paper represents part of a research program on some of the elementary conditions of human variability, which was initiated by the support of the Ernest Kempton Adams Research Fellowship of Columbia University. It is printed separately by permission as a part of the research activities of the Committee on Vestibular Research of the National Research Council. It is a continuation of the following series of studies of eye-movements, from photographic records.

'The Angle Velocity of Eye-movements,' in collaboration with Thomas Sparks Cline; *Psychological Review*, 1901, 8, 145-157.

'Five Types of Eye-movement in the Horizontal Meridian Plane of the Field of Regard, *American Journal of Physiology*, 1903, *8*, 307-329.

'An Experimental Study of Visual Fixation,' *Psychological Review*, Monograph Supplements, 1907, No. 35.

'An Analysis of the Involuntary Eye-movements in a Case of Congenital, Binocular, Lateral Nystagmus, from Photographic Records,' in collaboration with Allen Ross Diefendorf; *Archives of Ophthalmology*, 1909, *38*, 1-7.

'An Experimental Study of the Ocular Reactions of the Insane from Photographic Records,' in collaboration with Allen Ross Diefendorf; 1909, *Brain*, 31, 451-489.

'Psychological Effects of Alcohol,' in collaboration with Francis G. Benedict; Carnegie Institution of Washington, Publication 232, 1915.

'The Laws of Relative Fatigue,' *The Psychological Review*, 1917; *24*, 89-113.

'A Mirror-recorder for Photographing the Compensatory Movements of Closed Eyes, JOURNAL OF EXPERIMENTAL PSYCHOLOGY, 1921; 4, 165-174.

'The Latent Time of Compensatory Eye-movements,' JOURNAL OF EXPERIMENTAL PSYCHOLOGY, 1921, *4*, 247-269.

modified by habituation, still remains a serious one in aviation, for reasons which will soon appear.

Furthermore, the close relation of vestibular stimulation to seasickness has long been a part of our practical and scientific traditions. It is common experience that habituation to a ship's motion greatly reduces the symptoms of seasickness and produces a kind of immunity which lasts for considerable periods of time. This immunity seems to be a fixed characteristic of seafaring persons and there is a popular belief that it is transmitted to their offspring. Whether this habituation belongs to the vestibular mechanism or not is a question of some importance in the prevention and cure of mal de mer. Analogous adaptation to rotation seems to occur in dancers and in others whose duty or pleasure involves repeated stimulation of the vestibular mechanism. It has already been proved in the case of the white rat.

The first scientific observations of habituation to rotation in man were published by C. R. Griffith in this Journal.[1] From a technical standpoint these observations were open to several criticisms. Both the compensatory eye-movements and the pseudo-compensatory movements of stopping were complicated by visual fixation. In our ignorance of the effects of vision on vestibular nystagmus it is conceivable that the decrease of nystagmus which he observed was really an increased control of eye-movements by visual fixation. Futhermore, the estimation of the number and extent of eye-movements depended on direct observation. Since it was only possible to observe the eyes in after-nystagmus, any estimate of the changes which may have occurred in the nystagmus of rotation was precluded. It also involved the personal equation of the observer, the possible influence of which could not be accurately checked. The present experiments involved the use of photographic records. They furnish striking and complete confirmation of Griffith's results together with important additional data.

It was, however, on the theoretical side that adaptation to repeated vestibular stimulation claimed our attention.

[1] Journal of Experimental Psychology, 1920, Vol. III., pp. 15-46.

The central vestibular organ is very high in the primitive cerebro-spinal stem. Its intimate connection with the consciousness of orientation and of movement is no less conspicuous than its far-reaching and profound effects on the entire skeletal musculature and on the secretions. It involves a complication of neural arcs, second only to that of the cerebrum itself. As far as our present techniques go, it is the most complex reflex mechanism which we can subject to experimental study. The effect of repeated stimulation of this mechanism is consequently of particular importance to an understanding of human variability. It was this theoretical significance that induced the writer to explore it as a part of an extended attack on the elementary conditions of human variability.

Moreover, there is evidence that the ocular compensation to rotation which originates in the vestibular mechanism is not adequate to give the eyes the exact angular velocity that the environment appears to have. Continuous accurate compensation seems to require continuous readjustments. Vision alone transforms the rough vestibular reflex to an adequate visual compensation. In no other known reflex does the control pass so regularly and obviously to higher systematizations. In no other reflex is the conflict between the primitive and secondary systems of control so easily followed by graphic records. Abstracting for the present from these significant complications of the reflex we would follow the fate of the ocular compensation during protracted rotation.

The Effects of Protracted Rotation

In a previous paper[1] we have seen that the eye regularly reacts to bodily rotation on the vertical axis by a compensatory movement, the latency of which is of the order of 50 sigma. This low latency proves the reflex character of the compensation.

Except for its shorter latency (pursuit latency is about four times as long), the compensatory eye reflex resembles pursuit reactions of the eye when looking at a moving object.

[1]'The Latent Time of Compensatory Eye-movements,' Journal of Experimental Psychology, 1921, 4, pp. 247 fol.

Like pursuit, the reflex to rotation is neither continuous nor invariable. It suffers periodic interruptions by rapid saccadic eye-movements which bring the eye back approximately to its primary position. These saccadic movements are in turn interrupted by the slower compensatory drift. In both reflex compensation and visual pursuit, the alternation of slow drift in one direction and of saccadic correction in the other direction constitutes the peculiar oscillatory eye-movements which are commonly known as nystagmus. How indistinguishable these two forms of nystagmus are, will be shown in a later paper from photographic records of the reflex nystagmus and pursuit nystagmus which were taken under identical experimental conditions. Our attention is claimed first by the meaning of the alternation.

For obvious anatomical reasons the compensatory phase of the reflex cannot be continuous. The eye cannot continue to rotate in any one direction in its orbit beyond limits which are set by the muscles that anchor it. Hypothetically, we might suppose the eye to compensate to the limit of its anchorage and then stay fixed in an extreme position. This would, however, be an inadequate adjustment. In the first place, it would be uncomfortable. In the second place, it would be visually useless, since an eye fixed in an extreme position would be no better off visually than an eye fixed in its primary or central position. Clear vision of a moving field requires identical angular velocities of the eye and the environment.

Since continuous rotation of the eye is impossible, the best practicable conditions for clear vision of the environment during rotation would be to interrupt the compensatory glides by saccadic corrective movements which return the eye to its primary or central position. We know that saccadic eye-movements are moments of practical blindness. Since these are ordinarily unnoted, the interrupted compensatory glides seem to provide continuous clear vision of the environment during rotation. These conditions are realized in ocular nystagmus.

If regular rotation continues long enough the hydro-

dynamics of the vestibule would lead one to expect a gradual decrease of effective stimulation as the fluids of the vestibule come to equilibrium. The point of equilibrium is reached when the vestibular fluids and vestibular walls have identical angular velocities. At that point one could expect the reflex compensation of closed eyes to disappear. As a matter of record, protracted, even rotation at some point always ceases to be a stimulus for the compensatory movements of closed eyes. It is theoretically rather important to know when this occurs.

This disappearance of the reflex in protracted rotation is on several counts an interesting adaptation. Unless the ocular compensation is useful, it simply dies out. Why not? Physiologically, the phenomenon parallels DuBois-Reymond's law of electrical excitation. It is an illustration of the utility of Verworn's general definition of stimulus as a 'change in the external vital conditions of a tissue.'

In connection with our general problem of human variability, this adaptation may be noted as a clear instance of what is probably true in a large group of cases when continuous action of a stimulus-form ceases to be a stimulus. In this case the cause of a change in behavior is presumptively mechanical. Persistence of the stimulus-form produces a hydro-mechanical equilibrium within the receptor itself. This cannot properly be regarded as a fatigue process, though it is a decreased capacity for stimulation following stimulation. The receptor contains a mechanism for adaptation to the stimulus-form and mechanically develops a refractoriness to it. As one clear instance of a presumptively numerous class, we will name it, peripheral sensory refractoriness, mechanical type.

While a quantitative study of this peripheral sensory refractoriness was not contemplated in our present investigation, we have some unambiguous relevant data. In slow rotation of approximately threshold angular velocity peripheral refractoriness sets in almost immediately. Indeed, within limits which are not yet clearly defined, it was possible to accelerate rotation so gradually that it was only sensed

when it stopped. In rotations with sudden onset which reached an angular velocity of 100° per second, peripheral refractoriness occurred in one well-documented instance within ten complete rotations. That is to say, in this record, which was made photographically by our mirror-recorder, the nystagmus of rotation gradually decreased in frequency and amplitude and disappeared within ten complete rotations. (See Plate I. Record I.) It is suggestive that the moment of disappearance of nystagmus nearly coincided in this instance with the introspection of the subject that he seemed to have stopped rotating while the environment including the electric motor was flying around him. This coincidence between the cessation of nystagmus and the cessation of the consciousness of rotation is not universal. There are regular exceptions which will be serious stumbling-blocks in the pathway of a thoroughgoing behavioristic psychology. They will be described in a later paper on the threshold of rotation. Moreover, there is no assurance that peripheral refractoriness in this case was uncontaminated by central factors. On the contrary, evidence will be presented later in this paper that there is an important central mechanism which regularly develops a kind of central refractoriness. If the physical characteristics of the vestibule are ever to be studied by the method of peripheral refractoriness, some experimental means of differentiating central and peripheral adaptations must be devised.

Peripheral adaptation to protracted rotation by reason of the vestibular equilibrium has one notable result. When rotation decelerates, the physical vestibular equilibrium is disturbed in the same manner that it would be by the onset of rotation in the opposite direction. The nystagmus of deceleration, after-nystagmus, as it is commonly although somewhat inaccurately called, is the behavioristic aspect of a consciousness of rotation which is more or less violent according to the suddenness of the deceleration. Both phenomena, the adaptation to continuous rotation and the nystagmus of deceleration, seem to point clearly to the inertia-momentum hypothesis of the origin of vestibular stimulation.

But the matter is not quite so simple. Suitable deceleration and acceleration may greatly modify the illusion and may produce phenomena which are difficult to explain on the Breuer-Mach tradition. Protracted rotation that accelerates and decelerates very slowly yields some remarkable phenomena.

The Effects of Slow Acceleration

In developing a device which duplicates the very slow rotation accelerations that presumably may occur in flying I was really influenced by a systematic question. Is it possible to accelerate rotation so slowly that the vestibular mechanism is not stimulated at all in protracted rotation? In line with our previous discussion, one might state the question, is it possible to accelerate rotation so slowly that peripheral refractoriness of the vestibular mechanism is reached and maintained irrespective of angular velocity?

The question still remains unanswered. It proved impossible in my laboratory to completely exclude sensory factors which arose outside the vestibule. Although reduced as far as practicable, sound and light changes still gave some clew to rotation. The effort to answer the question, however, led to the discovery of unsuspected facts.

A suitable mechanical device for producing slow acceleration was not found at once. It may be generally useful. The specifications for such a device, as I saw the needs, were as follows: (1) A motor that would give no clew to the subject when the rotation began or how it progressed. (2) From a sub-threshold onset the speed of rotation must be slowly increased to a sustained or experimentally modifiable maximum. (3) To these primary specifications was added the desirability of putting the motor under the control of the subject so that he could modify its action either as to speed or direction in accordance with his perceptions and the instructions of the operator. While this feature of the apparatus was not taken advantage of in these particular observations, it was hoped that it would simulate closely the flier's control of his plane. The errors of control that occurred in connection with the illusions which we will presently describe

are highly instructive and should have a direct bearing on the art of flying. Specifications *1* and *2* seemed to preclude a fixed motor and to require that the motor revolve with the chair. Specification *1* precluded a gradually increasing motor speed. The change in pitch of motor noises might easily be a clew to the facts of rotation.

A simple and quite adequate answer to the specifications was finally found by mounting an electric fan on a low shelf-offset from the revolving platform, sufficiently eccentric to give it the requisite leverage. The control of speed and direction of motion was provided for by screwing the fan-base to a horizontal wooden pulley in such a manner that its direction could be modified by a steering-wheel which was conveniently located in front of the subject. The steering-wheel and the horizontal pulley were connected by a belt. The subject was protected from the air currents of the fan by suitable screens.

In using this device, the fan was started when the experimenter closed a switch in the fan circuit. At some point in the acceleration of the fan, if the chair was not intentionally held, the pressure of the air currents set it in motion. The onset of rotation which was produced by this device was never perceptible to the subject. Only after the platform had moved through a considerable angle was the motion sensed. Unfortunately the acceleration was never exactly alike in two successive experiments. Slight irregularities of various origin prevented mechanical exactness in the effects of so slight a force applied to so large a mass. Nevertheless, under similar conditions of direction of the fan and distribution of weight on the revolving platform, successive experiments showed similar accelerations. Rotation started very slowly. The first rotation took from twenty to twenty-six seconds.

Maximum acceleration occurred within the first rotation, a maximum speed between the thirtieth and fiftieth rotation. From then on the platform maintained its maximum angular velocity, 120°-180° per second, according to direction of the fan, with minor variations, until the fan was stopped or a

brake was applied to the platform. Long before maximum velocity was attained the subjects regularly reported some amazing illusions.

When the onset of rotation was first sensed by the subject he usually perceived its direction and acceleration. Occasionally, however, a subject reported that he was moving without clear direction. It is impossible at present to further describe this 'aliveness' as one subject called it. It is not likely to prove to be a mechanical product of the apparatus since the same report sometimes occurred when there was no physical rotation and in the after-image period. If the subject's eyes were closed, as was the regular procedure in our experiments, rotation with gradual onset always seemed very much less rapid than the physical fact. One reason for this is fairly obvious. Our rotation started at 0° per second and only reached super-threshold velocity after about half a rotation. Acceleration, which is the chief vestibular stimulus, started far below threshold value and never reached a value much above threshold. Under these circumstances, it is not surprising that the speed of rotation was underestimated.

Somewhere between the first and the tenth rotation, while the platform was yet accelerating, the subject reported that the platform was coming to rest. A little later it seemed to come completely to rest, and then suddenly still during physical, positive acceleration, the direction of rotation seemed to be reversed, and the subject seemed to rotate in a direction just opposite to that in which he actually went. This illusion was reported by all but one of more than twenty-five persons who were rotated in this manner. In most cases, it was perfectly clear and unmistakable. About fifteen of the experimental psychologists who met for a Spring meeting at Middletown, without exception reported a positive illusion. Various members of the Wesleyan faculty, members of the Executive Committee of the Vestibular Research Committee, and various guests of the Laboratory, physicians and other scientists, with one exception, reported the illusion. The illusions of stopping and reversal during acceleration seem inexplicable on the Breuer-Mach hypothesis. There is no physical explanation

of the phenomena in the behavior of fluids during gradual acceleration. The first illusion, that of stopping, might perhaps be understood as due to the reduction of positive acceleration below a threshold value, but that could not account for the illusion of reversal of direction. The question naturally arises, if there is perhaps some mechanism for adaptation to acceleration as well as a mechanism for adaptation to prolonged even rotation. The question seemed difficult to answer experimentally but we believe there is evidence for a central compensatory mechanism.

The illusion of reversed rotation gradually disappeared before the platform reached its maximum velocity. From then on, however long the subject was rotated, he seemed to be stationary, or to move only slightly in the one direction or the other. Fortuitous noises in the room seemed to revolve about his head with unaccountable speed. At no time was there any subjective appreciation of the actual speed of rotation. One subject, after about fifty rotations, said it seemed to him as though he had only 'gone around about three fourths of a circle.'

In order to produce satisfactory slow acceleration, the bearings of the platform must be very delicate and the balance must be quite accurately maintained. Under these circumstances, when the current to the fan is shut off, the deceleration also is slow and gradual. The illusion of reversed rotation, which is supposed to be an illusion of stopping, was found to be the regular result of deceleration. It was commonly somewhat delayed in slow deceleration, paralleling the delay in sensing slow positive acceleration. After its onset the illusion passed through a period of maximum effect and gradual diminution. The moment of physical stopping was seldom indicated by any peculiar subjective effects. Often the subjective stop came long before the physical.

Six to ten seconds after the apparatus stopped most subjects reported an illusion which one might call a positive after-image of rotation. Its direction was always the same as the original rotation. This passed through a cycle of increase to a maximum and gradual decrease until the subject

again reported himself to be motionless. In about half the instances there was a still later illusion of rotation. Whenever it occurred it was always in the opposite direction to that of the original rotation. One might call it a negative after-image of rotation. These two after-images, it should be noted, occurred while the subject was in fact absolutely still. The positive after-image was commonly clear and unmistakable. The later negative after-image was of a relatively small apparent angular velocity. It was often far from clear. Further faint illusions of rotation sometimes occurred but they were without regularity or compelling clearness. They seemed to depend on fortuitous circumstances such as attention to movements of the eyes and the like. There is no previous account known to the writer of any of these after-images. Like the reversal of direction during acceleration, the positive and negative after-images while the subject is at rest seem utterly inexplicable on the basis of the Breuer-Mach tradition. They, too, seem to indicate the interplay of some central neural agency whose nature we do not know.

In order to study the sequence of events more in detail a number of kymographic records were taken. They show the duration of each rotation and the correlated reports of the subject. As far as these records go no regularity appears in the illusions of acceleration after the first sequence of apparent stopping and reversal until we come to the illusions of deceleration. The data of a single experiment of fifty-two rotations are shown in Table 1. In this experiment a band-brake was used to vary the speed.

From the standpoint of interest in problems of aviation it is significant that absolutely the only correct report of the series was the first one. Even that was delayed. For the subsequent fifty turns every vestibular datum is false and would lead to disaster if it were permitted to control the pilot.

The case of slow acceleration is not unique in this respect. If one will recall the case of protracted even rotation it will be remembered that after a longer or shorter period of rotation, rotation seemed to the subject to cease. Though actually rotating as rapidly as before he seemed to be stand-

ing still. When rotation actually ceased he seemed to be rotating violently in the opposite direction. This illusion expresses itself in after nystagmus.

TABLE I

SLOW ACCELERATION EXPERIMENT

No.	T	V	A	Report	No.	T	V	A	Report	No.	T	V	A	Report
1	24″	15°		Right	21	3.9″	92.4°	2.4		41	2.9″	124.1°		
2	12	30	15	Still	22	3.8	94.7	2.3		42	3.2	112.7	−11.4	Left
3	10	36	6		23	3.7	97.4	2.7		43	3.3	109	−3.7	
4	8	45	9		24	3.5	102.8	5.4		44	3.5	102.8	−6.2	
5	7	51.4	6.4	Left	25	3.4	105.8	3.0		45	3.8	94.7	−8.1	
6	6.5	55.4	4		26	3.3	109	3.2		46	4.2	85.8	−9.9	
7	6	60	4.6		27	3.2	112.7	3.7	Left	47	4.6	78.3	−7.5	
8	5.8	62.1	2.1		28	3.1	116	3.3	Still	48	5.0	72	− 6.3	
9	5.5	65.5	3.4		29	3.1	116	0		49	5.5	65.5	− 6.5	
10	5.3	67.9	2.4		30	3.2	112.7	−3.3		50	6.5	55.4	−10.1	
11	—	—	—	Still	31	3.1	116	3.3		51	9	40	−15.4	Still
12	5	72	—		32	3.0	120	4		52	not completed			
13	4.7	76.7	4.7		33	3.0	120	0						
14	4.5	80	3.3		34	2.9	124	4	Left		10″ later right			
15	4.4	81.8	1.8	Left	35	3.0	120	−4			6″ later violent right			
16	4.3	83.8	2.0		36	3.0	120	0						
17	4.2	85.8	3.0		37	2.9	124.1	4.1			18″ later still			
18	4.2	85.8	0.0		38	2.9	124.1	0			30″ later left			
19	4.1	87.8	2.0		39	3.0	120	−4.1			14″ later still			
20	4	90	2.2	Still	40	—	—	—			No further report			

No. shows the number of the rotation.
T shows the duration of each rotation in seconds.
V shows the average angular velocity of each rotation.
A shows the average acceleration between each rotation and the preceding.

If one translated this experience into a situation in the air it would mean that after involuntary rotation of some duration the vestibular data would indicate that all rotation had ceased. If the involuntary rotation then actually ceased by accident or design, it would seem that the plane was rotating violently in the reverse direction. Unless compensated in some way such data would be disastrous.

HYPOTHESIS OF A CENTRAL COMPENSATING FACTOR

In the hope of developing a plausible working hypothesis with respect to the nature of a possible central factor to account for the illusions we plotted the subject's reports against the physical facts of rotation and the probable data

from the peripheral vestibular mechanism. When this was done it appeared that a single opposing central tendency would account for the illusions of cessation of rotation and reversal of motion during the early acceleration. A similar interplay of opposing after-effects would explain the sequence of positive and negative after-images. It was only necessary to assume that the curves of onset of the opposing forces have different radii so that they intersect twice. The graphical representation of this construction is given in Fig. 1.

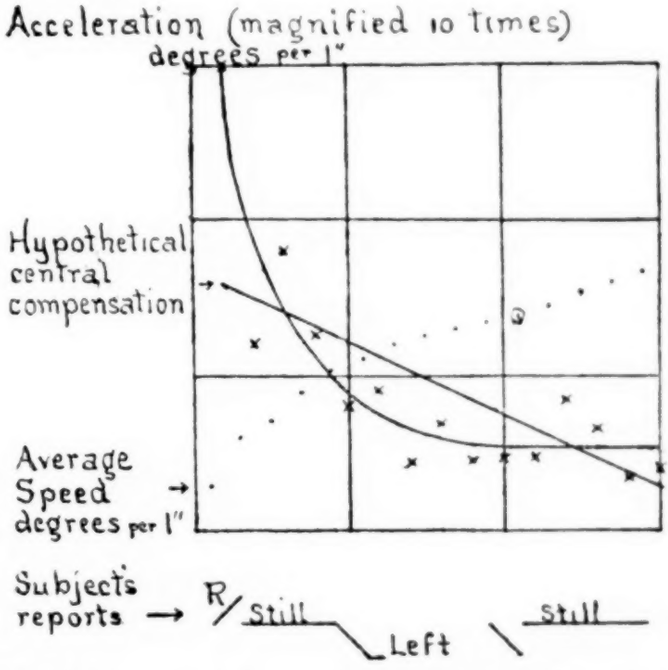


FIG. 1. Hypothetical interplay of central and peripheral factors. ⊙ indicates an interpolated value where the record was cut.

We have no conclusive proof of this hypothesis but there is some evidence in modern psychology that a stimulus not only tends to produce its normal accompaniment in consciousness but that it also sets in operation some antagonistic force that tends to inhibit the normal conscious accompaniments. This tendency finds expression in Kempf's law. Examples are numerous in the operation of effectual inhibition. In physiology it appears as the reciprocal innervation of antagonistics. Under our experimental conditions it is noteworthy that at no time was the actual speed of rotation or rate of acceleration a matter of consciousness.

It is not impossible that the relatively faint stimuli from the vestibule, which are produced by very slow acceleration, were made still less effective by a central inhibiting factor, such as we have suggested.

An experimental effort to test this hypothesis was made as follows: I reasoned that if a central compensating factor really existed, it should be possible to inhibit or neutralize it experimentally. If this were done then both the illusion of reversal and the after-images would disappear according to hypothesis. In consequence of this presumption we tried rotating a subject in the usual manner but with the eyes open. Under such circumstances consciousness of the speed of rotation is inevitable. He sees the field of view rotating around him. These experimental conditions, however, did not prevent the reversal of the point of reference. That is to say, during the protracted rotation, even with the eyes open, there was always a point when the rotation of the subject seemed to cease and when the environment seemed to rotate around him. This we assume corresponds to the moment of peripheral adaptation. The vestibule ceases to send in rotation-stimuli. Since vestibular equilibrium indicates to the subject that he is not moving, the obvious motion is referred to the environment. There was no other illusion or after-image. As far as it goes, this verified prediction must be regarded as a confirmatory indication of our hypothesis. It seems to us an interesting line of investigation with indefinite points of bearing in normal as well as in abnormal mental life. As an example of what is possibly a considerable group of such central compensations we may call this hypothetical central factor, central compensation; and the consequence of its action, central refractoriness.

It is of special significance that, under the circumstances of these experiments, the records might reach a speed of 180° per second, while a velocity of 120° per second was continued up to one hundred rotations, absolutely without vertigo, nausea, or any disagreeable after-effects whatsoever. To those who were accustomed to the regulation rotation tests the experience was a great surprise. We rotated sub-

jects who were otherwise very susceptible to rotation and easily made dizzy, without any unpleasant after effects. Even when the rotations were made with the eyes open there was usually no discomfort. If, however, the head was raised from the head-rest during or immediately after rotation or if it were otherwise moved very distressing symptoms might appear, which in one case lasted several hours. The inference is inevitable. Uncomplicated and undisturbed slowly accelerating rotation does not cause vertigo.

Effects of Repetition of Rotation

Contrasted with the effects of protracted rotation, repeated rotation experiments introduce us to a different set of problems. This is especially true when the repetitions of rotation are separated by intervals which are sufficiently long to permit the peripheral mechanism to attain complete equilibrium. Under these circumstances every modification of reaction and of consciousness which is effected by successive rotations must be due to changes which are commonly grouped under the concepts of habituation and learning. The very existence of habituation to rotation was vigorously denied during the war by those who were responsible for the revolving chair tests for prospective aviators. We may say that the mechanism of such habituation, if it exists, is quite unknown. Our methods of recording the eye-movements from closed eyes not only permitted study of this phenomenon in its pure form but enabled us to make an experimental study of a mechanism which, because of its peculiar position in the central nervous system, we may regard as a primitive human learning process.

For the sake of clearness and conciseness, we will not attempt to follow the historical development of the experiments but will describe at once the most extensive course of training which we undertook. This experiment disclosed the phenomenon of habituation to rotation beyond the possibility of accident, permits a quantitative statement of the course of training, and indicates the principles by which the effects of repetition in other series may be understood. The

essentials of the experiment consisted of a six-day training period during which the subject (myself) was rotated in the same direction one hundred and fourteen times each day at as nearly uniform speed as possible. In the hope of increasing the practice effects as well as for the convenience of the photographic technique, the daily practice was divided into nineteen series of six rotations each. Each series began two minutes after the preceding one. The rotation speed was 120° per second or three seconds per revolution. The first and thereafter every alternate series of six rotations up to the nineteenth, were photographically recorded.

Each photographic record[1] shows the following data: (1) time ordinates at intervals of two seconds, (2) the beginning and end of rotation, (3) each completed half-rotation, (4) the number and extent of eye-movements, (5) the number and extent of head-movements. Plate II reproduces three significant records of these experiments. They are taken by a Dodge-Cline falling plate recording-camera through a horizontal slit.

The time ordinates were made by the flash of a straight filament lamp every two seconds. This occurred when a seconds-pendulum at one end of each left-hand swing momentarily short-circuited a high resistance in series with the lamp. The resistance was adjusted to saturate the filament and produce a non-actinic red glow. This had no noticeable effect on the record. At each flash an elongated image of the filament was photographed on the sensitive record through the recording slit.

The beginning and end of rotation is shown by a line (Rotation Indicator), which was recorded by a concave mirror which remained fixed in space as the platform revolved around it. The device for keeping this motionless has already been described.[2] It consists in principle of a vertical shaft whose upper axis lies in a line directly above the axis of rotation and whose lower end is geared to the axis of the platform in such a way that the shaft makes one rotation relative to the

[1]Plate II., Records 1, 2, 3.

[2]Jour. of Experimental Psychology, 1921, *4*, 251.

platform at each rotation of the platform. Since the directions are reversed by the driving belt the shaft really remains motionless while the platform rotates around it. Such a device might reasonably be expected to record each rotation of the platform as well as the beginning and end of rotation. Unfortunately, at the speed used in this experiment, the recording beam of light proved to be too faint for such a purpose. In order to record each rotation we considered the following alternatives: we might either enlarge the mirror-surface of the onset-recorder or develop an independent rotation-indicator. Since large mirrors tend to produce heavy, blurred lines which are difficult to read we chose the latter alternative requiring a separate indicator of rotation. For this purpose a separate low voltage lamp was placed in front of the camera slit. It was screened in such a way that it could illumine only a small area at the left-hand of the record. It was connected in series with contacts on the revolving platform and the previously mentioned geared rod so as to produce two flashes of unequal duration at each rotation. The consequent records appear as elongated spots at the left of each photographic record. Eye and head-movements are recorded by the mirror-recorder whose construction and operation are described at length in a previous paper.[1]

In this training series the platform was rotated by a $\frac{1}{6}$ h.p. electric motor to which it was connected by a belt. With the help of an elastic spring at the onset of rotation, maximum velocity was reached within the first quarter turn. After $5\frac{1}{2}$ rotations the motor was shut off simultaneously with the application of an automatic brake. The actual stop was produced by friction between two velvet surfaces. One was carried by an offset-arm extending horizontally from the platform. The other covered an inclined plane which rested on a heavy oak table. After the motor circuit was cut the inclined plane was moved into position to be struck by the arm. These surfaces stopped the platform within a few degrees of the same spot in successive experiments. This was usually sufficiently accurate to bring the record of stop-

[1] JOURNAL OF EXPERIMENTAL PSYCHOLOGY, 1921, Vol. IV., pp. 165.

ping on to the photographic plate. This combination of brakes operated quietly and without annoying jar. In Plate II., Line *RI* gives a graphic record of the stop as well as of the onset of rotations in Records 2 and 3. In estimating the speed of onset from the records, one centimeter displacement of the record line corresponds to 1° of rotation. In all cases, the direction of rotation was counter-clockwise.

A graphic summary of the measurements of the total compensatory phases of the nystagmus of stopping under these circumstances is given in Fig. 2. Each line shows the total extent of the slow compensatory phases of the nystagmus by two second intervals. Time is represented on the abscissa; the sum of the slow phases of nystagmus on the vertical ordinate.

Inspection of the curves shows that the amplitude of post-rotation nystagmus (after-nystagmus) decreased from day to day throughout the experiment. There was also a tendency for the amplitude to decrease from record to record within each experimental day. Within each record the amplitude decreased with the lapse of time after rotation ceased.

The decrease of amplitude within each record tends to follow a fairly well defined curve. It proceeds more rapidly at first, then more and more slowly. All the experimental days show decrease in the amplitude of nystagmus from record to record. In some series it is more rapid than in other series and each series shows some irregularities. But the irregularities of the curves do not obscure either tendency. The daily decrease in the amplitude of nystagmus is equally conspicuous. The first record of each day stands at a lower level than that of the day before. At no point during the first three days does the post-rotation nystagmus disappear within the period of time which is covered by our records. During the last three days of experiment, on the contrary, all records after the first show that the amplitude of nystagmus reached zero.

Inspection of the record which is reproduced in Plate II., Record 3, shows that when the nystagmus disappears there is

a residuum of movement or drift in the direction of pseudo-compensation. That is to say, after the nystagmus com-

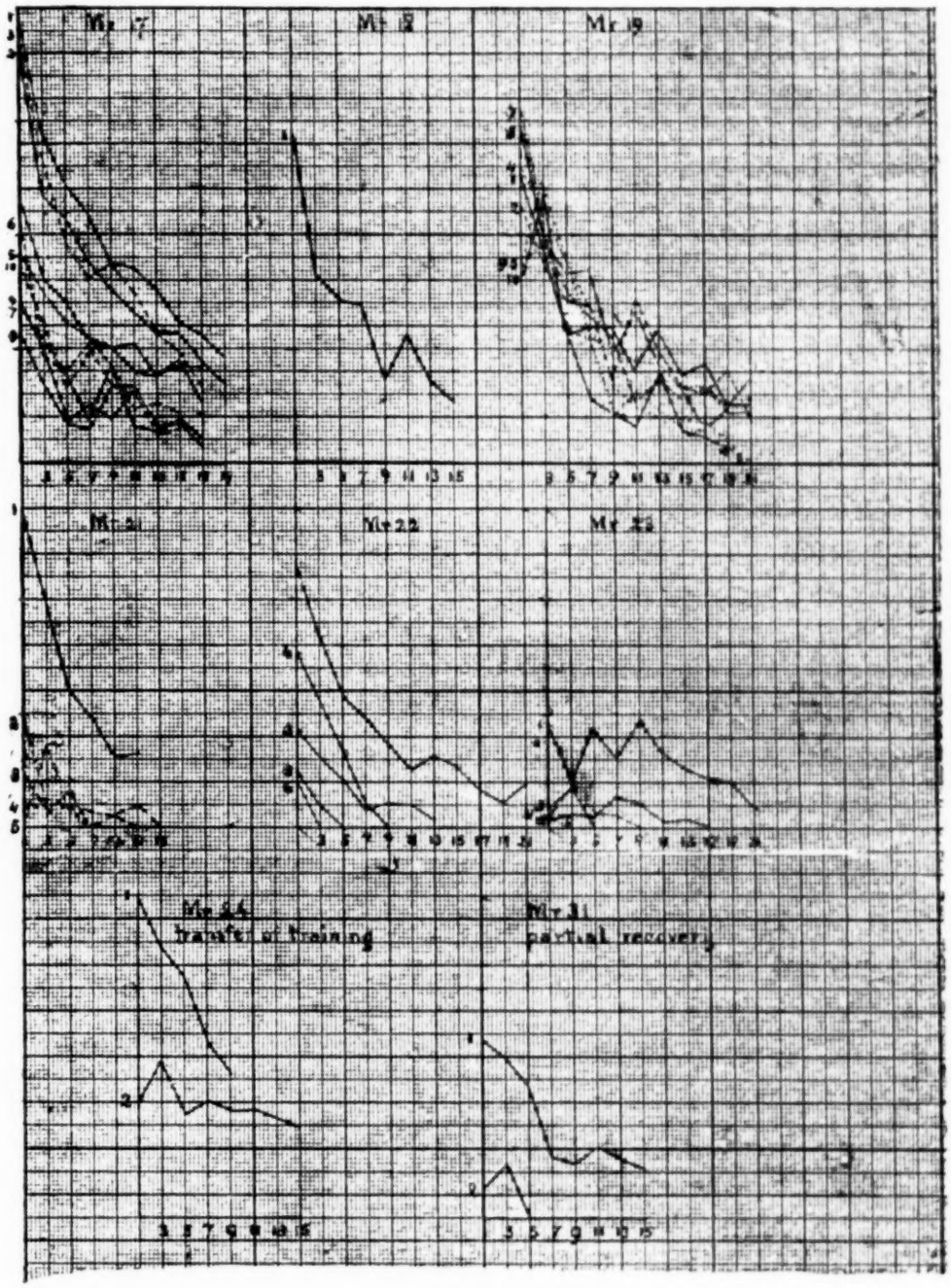


FIG. 2. Decrease in the nystagmus of stopping (after-nystagmus) during a training series and after.

pletely disappears an uncorrected compensatory drift may still remain. This is usual in all records of the training series. In terms of susceptibility to training, it appears

that the saccadic phase of nystagmus disappears before the compensatory phase. This seems to be in direct accord with the scientific tradition that the two phases originate at different points in the nervous system. The greater susceptibility of the saccadic phase to habituation would substantiate the supposition that its point of origin was higher. We have already noted evidence that the compensatory phase is a true reflex to rotation. The saccadic phase, on the contrary, gives no indication of being caused directly by rotation. These movements rather appear to be corrective reactions to the reflex displacement of the eye. They represent a mechanism for returning the eyes to approximately their primary position after reflex displacement. Measurements of the temporal incidence of these saccadic phases showed great variability. Their latency varies from a few sigma to more than half a second. Occasionally the nystagmus degenerated at the end of a record into a series of fine oscillations without the characteristic nystagmoid bias. More rarely one finds short periods when the direction of nystagmus appears to reverse. Unfortunately, we have no means for correlating these degenerated forms of the nystagmus of stopping with the subjective phenomena of apparent rotation.

The next day after the training series ended we took a series of records exactly like the training series except for the last two. In these two records, the direction of physical rotation was reversed. We hoped in this way to discover whether the habituation to rotation had transferred from the counter-clockwise rotation of the main experiment to clockwise rotation. The resulting curves correspond with the curves of the mid-training period. They indicate that about half the training in one direction had transferred to the other. The transfer of training to other speeds was not investigated. Our records contain incidental proof that it was only partial.

One week later, we took six records in the same manner as during the training period and in the same direction. It was hoped by means of these records to learn how much, if any, of the training had been lost in the intervening time.

The first record of this post-training series lies between the first records of the last day and that of the next to the last day of the training period. The second and succeeding records would rank with the best records of the main experimental period. That is to say, while the first record of this post-training series showed a slight loss of training, this loss was immediately made up in the subsequent records.

The introspective data of the training period give some evidence with respect to the subjective phenomena of habituation to rotation. During the first session the rotation experiments were more or less unpleasant. The subjective report at that time stated that the second record was the record of most dizziness. Before the end of the first session, however, the subject felt that he had developed entire indifference to the rotation and would have said that there were no eye-movements at all except those immediately after stopping. This subjective indifference to rotation developed towards the end of the training period into a striking lack of clear consciousness of rotation. The report at the last session is as follows: "During this entire experimental session I have had very little of the usual consciousness of motion. The rotation feels peculiarly natural and comfortable." This comfortableness was a very conspicuous phenomenon of the later series. It can scarcely be over-emphasized how completely the character of the subjective reaction to rotation changed. Instead of being a disagreeable task the rotation experiment had a soothing, soporific character, both during and immediately after rotation. This peculiar pleasantness or comfortableness carried over into the post-training series. It did not carry over into reversed rotation or accidental interference with the customary speed.

The course of training, the transfer of training and the slight loss of training by intervening time all show the familiar phenomena that we have long associated with learning. There can be no doubt that under the experimental conditions of our training period the pseudo-compensation reflex of the eyes to stopping (after-nystagmus) suffers a characteristic habituation effect. The amplitude of the reflex is greatly

diminished and the nystagmus of deceleration completely disappears. A closely analogous course of habituation is shown in the compensatory eye-movements of acceleration. Something of this will be observable directly from inspection of the records of Plate II. A quantitative statement of its progress has not been attempted, since some of the more important records are defective and difficult or impossible to read accurately. Unfortunately, our rotating contact, which supplied the current for the recording light, functioned unevenly in the early rotations. The conditions of the experiment prohibited our repeating defective records. There is, however, one clear difference between training of the nystagmus of stopping and training of the nystagmus of acceleration. Whereas in the former case the nystagmus completely disappeared, in the latter case, traces of it were always present at the beginning of rotation.

Habituation to Oscillation

Additional data on the training of compensatory eye-reflexes are shown in a little greater variety by an earlier series of experiments on adaptation to oscillation. The use of oscillatory instead of protracted rotation in one direction was based on the supposition that a series of oscillations would produce the greater inertia-momentum changes and consequently the greater stimulation of the vestibular mechanism.

Six laboratory students, in addition to myself, served as subjects in these experiments. Rotation records as well as the records of eye-movements and head-movements were made in this series much as in the longer training series. The oscillations of the subject were produced as follows: A group of light rubber bands about four inches long was stretched from an offset-arm which projected from the rotating platform, to a firm support on an adjacent table. When the platform was rotated so that the tension of the rubber bands was increased and then suddenly released, the force of the stretched rubber bands produced a series of damped oscillations of the platform and gradually brought it to rest. The

extent of the initial oscillation was kept constant by an artificial stop against which the offset arm was pressed before its release. The frequency of oscillation depended primarily on the number of rubber bands and their tension but it also depended on the balance of the platform and the relative position of the subject. These conditions were roughly adjusted empirically to an approximate standard. Accurate adjustment would have meant preliminary oscillation of each subject before each series of rotations. This was obviously impossible in a training experiment such as we conducted. In these series, twelve records usually constituted an experiment. One series was of double length. There were no unrecorded series of oscillations. Subjects were instructed to fixate attention on a corner of a house across the street from the laboratory, and to keep the closed eyes pointed in that direction as consistently as possible. This was naturally very imperfectly done but it usually served to keep the eye-line from wandering off the photographic record.

The records of these experiments show that each subject had his own individual peculiarities of eye-movement. No two subjects showed the same changes in successive records. One general tendency, however, appears in the successive records of all subjects.

The first records from each subject usually show more or less irregular wanderings of the line of regard, which are not connected with compensation to rotation or with its corrections. Some of these wanderings are probably movements of convergence or divergence. Some of them are voluntary efforts to correct supposed lapses in the imaginary fixation. Some of them are possibly nervous hyper-activity. These useless irregularities commonly disappear in successive records. They give place to more simple forms in which true compensatory drifts are interrupted by corrective saccadic movements. These often take the form of perfectly regular nystagmus which changes the drift of the compensatory phase with each change in the direction of rotation. Characteristic records are reproduced in Plate I., Records 2–4.

The accuracy with which the change in direction of com-

pensation may occur is conspicuously shown in Plate I., Record 2.

Occasional anticipatory compensation as in Plate I., Record 3, suggests the interaction of cortical motor habituation. In successive records there is a further general tendency for the gross nystagmus to disappear in relatively smooth compensation curves. This was especially true when, as in my own case, the subject made an effort to keep his eyes quiet. Plate I., Record 4. In no case was the training process carried as far as in our regular training experiments. Compensation, consequently, never disappeared although it was frequently much reduced in amplitude in the last records of a series.

These records of eye-movements during oscillation, like the records of the main series, indicate a true habituation process, which may be summarized as follows: Useless saccadic movements disappeared first. The first stage of habituation in this case increased the accuracy of the true compensation process by inhibiting intercurrent disturbances. The next obvious change in successive records is the diminishing amplitude of the corrective saccadic movements. This finally transforms the nystagmus of rotation into relatively smooth compensatory glides. These in turn show a tendency to decrease in amplitude as habituation to rotation progresses.

Degeneration of Coördinate Compensatory Eye-movements as the Effect of Repetition

Training produced a much more rapid and profound change in the character of eye-movements of the coördinate compensatory type than it produced in reflex compensatory movements. The distinction between these two types of eye-movements may be recapitulated as follows:

Reactive compensatory eye-movements are reflexes to stimulation of the vestibule or semicircular canals. They have a latency of the order of 50 sigma. Coördinate compensatory eye-movements have no fixed latency. They often occur without any latency whatsoever and are coör-

dinate with voluntary movements as their name implies. That is to say, when we look at an object and rotate the head clockwise, the eyes maintain fixation by rotating counter-clockwise at the same instant through a similar angle. The character of these eye-movements and their lack of latency have already been proved by photographic records.[1]

In that paper a device was described for the mechanical elimination of the head-movement component in records of compensatory eye-movements of this type. The device there used was a system of light wooden levers, one end of which was held firmly in the mouth while the other end carried the sensitive paper behind the slit of the recording camera. Unfortunately, as was explained in this connection, the system of wooden levers had an inertia-momentum component which slightly distorted the records. It is doubtful if a mechanical system can be constructed which would be entirely free from these errors. The hope was then expressed that it might be possible to construct an optical system for the same purpose. The advantages of the optical system are obvious. A beam of light has neither inertia nor momentum. The construction of such an optical system constituted a beautiful technical problem. The specific problem may be stated as follows: Our mirror-recorder for registering the movements of eyes under closed lids consists essentially of a minute concave mirror held by a suitable framework and resting on the closed eyelid over the cornea of the eye. Movements of the eye underneath this mirror rotated it from side to side and deflected the recording beam of light. The difficulty in recording coördinate compensatory movements consists of the fact that rotation of the head which conditions this type of eye-movements also displaces the eye-recording beam. Our problem was to neutralize the displacement which was due to movement of the head without affecting the displacement that was due to movement of the eye. It will be clearly seen that the technical difficulty was a real one.

Analysis of the technical problem led to the following considerations. A concave silvered sphere would reflect the

[1] JOURNAL OF EXPERIMENTAL PSYCHOLOGY, 1921, Vol. IV., pp. 255–258.

light which emanated from its center back to the central point of origin. Similar relations would hold for any fragment of such a sphere rotating at the end of a rod whose length from the axis of rotation to the attachment of the mirror was the length of the radius of a sphere. This optical construction was translated into terms of head-movement as follows: If the source of illumination and the slit of the recording camera were both placed on the vertical axis of the head, a plain mirror parallel to the axis of rotation and perpendicular to its plane of rotation would reflect a small recording beam to the slit without displacement, irrespective of the angle of head-movement. The problem was theoretically solved. It only remained to devise a convenient framework to hold the mirrors and to provide a fixed vertical axis of rotation of the head.

The general plan of construction was as follows: A recording camera for film or sensitive paper was built so compact that it could be adjusted to clear the top of the subject's head above its axis of rotation. In order to stabilize the axis of rotation of the head and to keep the axis vertical, a socket was attached to the top of the head by a broad elastic band. When the socket engaged a rounded offset, which was placed just below the slit, the axis of the head was held fairly rigidly in line with the slit of the recording camera. To bring the recording light into this same line proved optically impracticable. Consequently, our lamp with its horseshoe filament was placed in front of the camera but as close to it as possible. This gave slight head-movement records but our results showed that the consequent slight distortion of the eye-line was practically negligible. The height of the recording light was adjusted to focus the image of its filament on the slit of the recording camera. The tangential mirror was divided into two mirrors to allow for the difference in height between the eyes and the slit of the recording camera. These two mirrors were carried on a light framework of thin wooden strips. The framework was held firmly in the mouth at its lower end. Its upper end was flexibly attached to a horizontal rotating arm whose axis

was in line with the axis of the head and the slit. This arrangement brought all the moving members of our construction into a single system corresponding to the optical system with which we started. The plain mirrors which were carried on the wooden frame served the purpose of optically connecting the concave mirror of the mirror-recorder with the superposed slit of the recording camera. For that purpose, they were adjustable so that one might be placed in the horizontal plane of the eyes, the other in the horizontal plane of the camera slit. Both were oriented to each other at an angle of 90° and at an angle of 45° to their respective planes.

.Starting with the horseshoe filament of the recording lamp one might trace the path of the recording pencil of light as follows: Suitably screened to prevent diffuse illumination of the room, the recording pencil of light was directed to the lower mirror, thence to the concave mirror of the mirror-recorder, thence back to the lower plain mirror, thence to the upper plain mirror, thence to the slit of the recording camera across which the recording image of the lamp-filament was formed by the concave mirror of the mirror-recorder. The particular device for holding these mirrors is an accident. It is only necessary to reproduce and maintain the geometrical relationship. The resulting records, 1, 2, 3, and 4, Plate III., prove the correctness of our theory and the adequacy of the device.

It will be noted that the head-line is not absolutely straight in these records. The slight displacements of the head-line during rotation of the head are due to two main circumstances. One of these circumstances was the slightly eccentric position of the recording lamp which we have already noted. This was a defect of our technique but its effects were so slight as to make questionable the indicated expensive reconstruction of our recording camera. The other circumstance was the irregularity in the shape of the filament. Doubtless, lamps could be constructed to overcome this defect of the filament. In the nitrogen lamps that we were able to obtain commercially the filament loop was never perfectly regular. Its irregularities, as well as the shape of the loop itself, gave

the recording image different configurations for different angles of incidence. Such changes in the configuration of the recording image made the record appear to move even when the main position of the image was unchanged.

With the technical elimination of head-movements from our records, the latter lost a factor that is really necessary for their interpretation. While it was desirable to eliminate the head-movement factor from the records of eye-movements, unless we have some record of head-movement there would remain no possibility of correlating an eye-movement with the head-movement for which it compensated. The technical problem of eliminating the effects of head-movements and yet retaining a record of them was solved by introducing an entirely new registration system into the record for the express purpose of showing the beginning, course, and end of head-movements. This new system shows in our records, Plate III., as longitudinal, white shadows. They are in reality shadows of the wooden framework that carried the mirrors. As the sensitive paper fell behind the slit of the recording camera, the movements of the shadow produced the white band records of head-movement. It will be remembered that the wooden frame was held firmly in the subject's mouth and consequently moved with his head. The illumination which produced the shadow originated in an alternating current arc lamp burning at six amperes. The light from this arc lamp was suitably screened to prevent diffusion in the room and was reduced in intensity by a number of layers of green glass.

The correlation between displacement of the shadow record and the angular displacement of the head was determined empirically. Within the range of our experiments ten degrees of head-movements produce twenty-six millimeters of shadow displacement on the record. On this basis, measurements of the records of Plate III. show that the voluntary head-movements averaged seven degrees in amplitude.

The super-position of this alternating current record served the additional purpose of a time record. Flashes

of the alternating current appear as dark lines across the record. Unfortunately, they were too close together to reproduce well. With an alternating current of sixty cycles per second the time lines are separated either by a sixtieth or a hundred and twentieth of a second. When both phases of the alternation appear on the record one phase is regularly fainter than the other. This follows from the fact that our arc lamp had ninety degree carbons, one of which was horizontal. In view of the slight inaccuracy of the alternating current cycle, accurate time records would probably demand still further control. This would be particularly true of long periods of time. For the short periods of time that were significant in our records the errors of the alternating current were negligible.

Inspection of the first record of Plate III. shows the accuracy with which this type of compensatory eye-movement was coördinated with head-movement when one of the eyes was open. Especial attention is called to the first right-hand movement reading from the bottom up. The amazing fidelity with which the compensation of the eyes is regulated to the vagaries of the head-movement was pointed out in our paper on the latent time of compensatory eye-movements. The accidental saccadic movements that are noticeable in Record 1 do not hide the accuracy of the compensatory glides. The second record of Plate III. was taken immediately after the first. The only difference in their conditions was that in the second record both eyes were closed while in the first record the right eye was open. In both cases the record was taken from the closed left eye. It is conspicuous that in Record 2 the eye line has become grossly modified. One must look carefully to find occasional parts of the curves which indicate true coördinate compensation. They are discoverable, however, coördinate with some part of every head-movement although all of them are disturbed by long and irregular inter-current saccadic movements. Records 3 and 4 are respectively the second and fifth records subsequent to Record 2. Both of these were taken with both eyes closed. In Record 4, two head-movements occur with no compensatory eye-

movements whatsoever. In view of the accuracy of coördinate compensatory eye-movements when they are under the control of vision and in view of their lack of latency, their rapid degeneration when the eyes were closed remains incomprehensible to us. If the coördinate compensatory eye-movement is centrally elaborated as a branch of the nervous process which initiates voluntary head-movement, one might expect the coördination to persist with closed eyes. If they need the control of vision for accurate adjustment one might expect a reasonable latency. The facts are directly opposed to what seems to be reasonable expectation. Immediately after the opening of the right eye, records return to the form of Record 1, without any measurable period of uncertainty. In our earlier discussion we found reason to believe that the coördinate compensatory eye-movements originate in the same voluntary motor system that moves the head, that is to say, in the cerebral cortex. Their rapid degeneration and recovery support this view. Such rapid changes in successive stimulation are not characteristic of lower neural mechanisms. Direct proof of this supposition must come from pathological cases. At any rate, a striking difference between habituation to rotation in the case of the reactive compensatory eye-movements and in the case of coördinate compensatory eye-movements is demonstrated by our records.

The Relation of Learning to Inhibition

In addition to the direct proof of habituation to rotation our records throw an interesting side-light on the elementary consequences of repetition. This is, indeed, their main claim to attention and was the theoretical consideration that inspired our experimental study. We have previously noted that the compensatory eye reflexes are conditioned by the action of a part of the nervous system which is relatively high on the primitive cerebro-spinal axis. It is not unreasonable to expect to find at this level premonitions of cortical developments—effects of repetition, which represent cortical learning processes in simpler configuration. In our training

[1] Journal of Experimental Psychology, 1921, Vol. IV., pp. 258–262.

experiments, each day showed slight improvement over the preceding day but the beginning of each day had lost something of the improvement that appeared at the end of the previous day. In this and other respects, our habituation curves do in fact correspond to the curves of the learning process. But, however striking the similarity, we must not be misled into a naïve assumption of identity. According to our scientific tradition the two processes are direct opposites. In ordinary learning some discreet reaction is gradually established and consolidated. In our habituation experiments a reflex gradually enters a pronounced refractory phase and is temporarily disestablished. The fact that the course of the latter process so closely duplicates the ordinary learning curve deserves careful consideration. It cannot be dismissed as accidental.

Notwithstanding notable advances in our scientific knowledge of the learning process, there is still considerable doubt as to the precise neural action which conditions it. The phenomena of behavior and consciousness are relatively clear. They are formulated in the traditional laws of memory and learning. But our psycho-physiological traditions with respect to the neural conditions of learning are purely hypothetical constructs. Decreased synaptic resistance, amœboid movement, growth, grooves in the neural structure, ingrams, etc., are current terms by which we express either hypothesis or ignorance. Such concepts are useful as working hypotheses. Unfortunately, they easily become fixed as dogma. It seems to us that our experimental results cannot be thoughtfully regarded without raising serious questions concerning our scientific tradition, which suggest a new attack on the neural problem of learning.

Three possibilities have been considered to account for the typical learning curve which is followed in the disappearance of the compensatory eye reflexes. It might appear simplest to assume that two antithetical processes, learning and the refractory phase, both follow a similar course. The moment one tries to translate this into physiology of the nervous system, however, it obviously leads to something else. If

repetition has a similar effect in apparently antithetical processes, it would seem that there must be some underlying similarity in the neural conditions. We commonly accept it as a truism that the same event cannot produce similar results unless there is some fundamental similarity in the system on which it acts. Our first suggestion consequently developed into a second.

It is possible that the neural conditions in both learning and the development of refractoriness have some fundamental neural factors in common. There is considerable evidence in modern physiology of the nervous system that the nerve action in stimulation, facilitation, and inhibition is not qualitatively different. The apparent antithesis in effect may be due to the peculiarity of the tissues which a given nervous impulse reaches. It is possible that the difference may be referred to the relation between the refractory phase of the tissue affected and the frequency of the stimuli-pulsations. The truth or falsity of these suppositions is a matter of physiology. However, we will all doubtless agree that the possibility of a common fundamental nerve process in stimulation and inhibition is not a physiological absurdity. Such a common fundamental neural process, if it exists, would have to be regarded as the universal residuum of stimulation. It would facilitate subsequent stimuli when its systematic connections favored facilitation. Conversely, it would produce a refractory phase in accordance with the nature of the system affected. In connection with our study of the elementary conditions of human variability, we shall return to this problem in a later paper with some further direct experimental evidence as to the nature of inhibition in human neural processes.

If our second supposition is true, that is, if there is some fundamental neural factor which is identical in the learning process and in the disappearance of a reflex by habituation, we are obviously led one step further to the assumption of the essential identity of the neural process in these two cases up to some point of their systematic connections. Students of animal behavior have given currency in our tradition to the

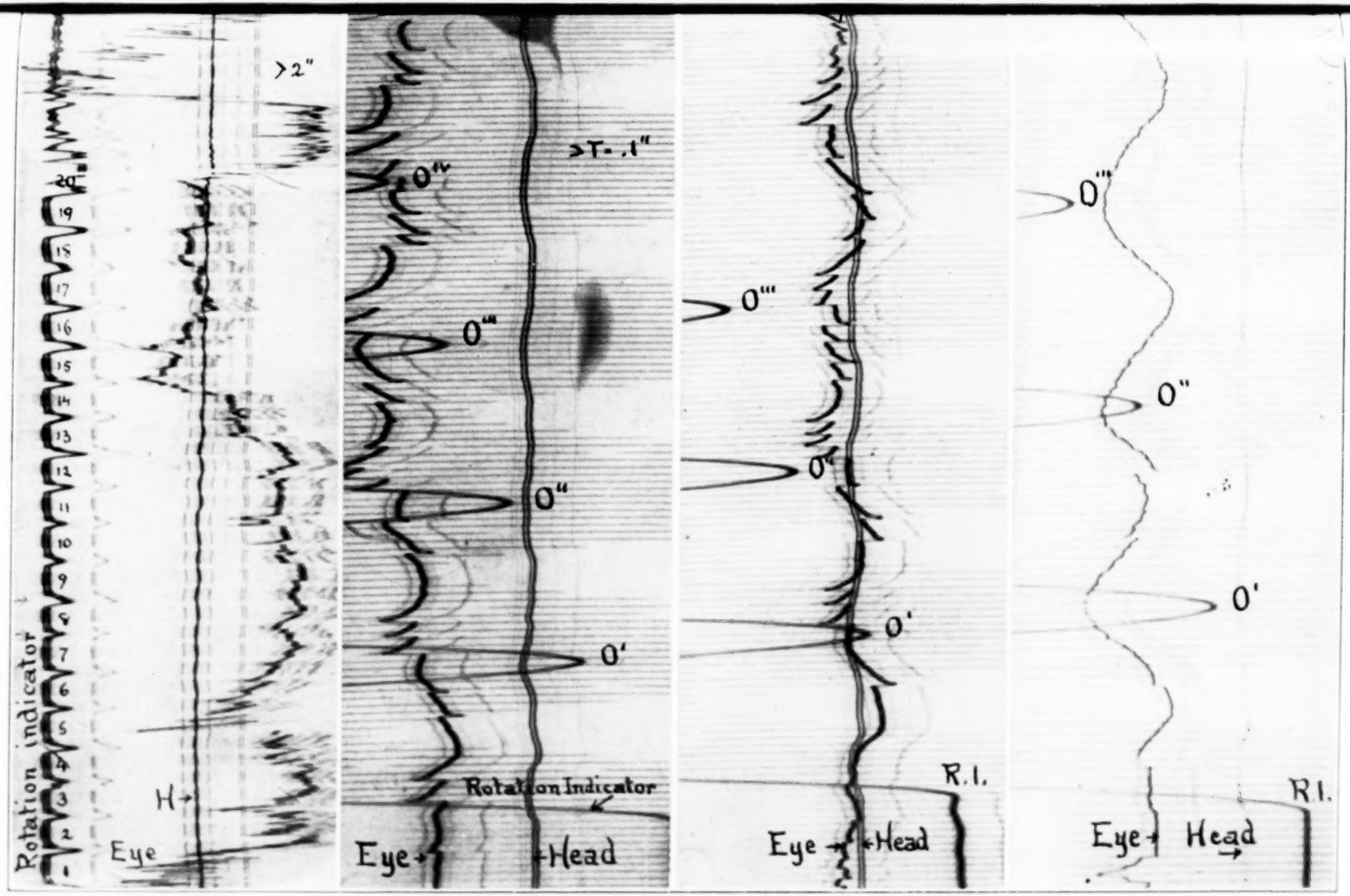
concept of trial and error. It is a descriptive expression for the fundamental learning process of animals. In animal life the elementary form of learning seems to be very largely the elimination of the useless or inappropriate modes of response from the repertoire of multiple responses to a repeated stimulus. In recognition of the epoch-making researches of Pawlow, the final outcome is commonly called a conditioned reflex. The reinforcement of productive modes of response may be relative rather than fundamental. That is to say, as the useless responses disappear, the useful response may appear to be more prominent not because it is really reinforced but because it alone is left in the field. The same fundamental process has been traced in many forms of human learning although the obvious neural implications have never been explicitly accepted, as far as I am aware.

Human learning always involves a negative factor. In some instances, the negative factor is dominant. The presence of a pool of water operates as a stimulus to a child for an indefinite multiplicity of reactions, such as wading, splashing, draining, navigating, etc. The training of the adult has added relatively little to his modes of reaction to the pool of water except by way of complication. It has, however, eventuated in the complete elimination of most of his early reactions. The wise adult simply avoids the pool and other things.

When one learns a verse of poetry beginning 'Listen my—' the specific learning process may add absolutely no new association between the word 'my' and its neighbors. The verse is learned when all other associations are inhibited and the association 'my children' is retained.

Learning a language, or learning history, seems to follow the same model amazingly closely. One does not look for a multiplicity of responses in spelling, grammar, geometry, or the identification of a flower. Education is in fact very largely made up of the elimination of undesirable responses. I beleive that it would notably clarify our educational thinking if the obvious consequences of our experiment led to a re-examination of the learning process.

The question, how new phases of response originate, is quite another matter. In the lower animals and in the lower nervous system of man, the artificial origin of really new modes of response is all but hopeless. Unless the animal makes the noises of human speech on his own initiative there is no hope of teaching him to speak. Unless the digits flex separately by some native mechanism the process can not be taught. Native modes of reaction separate the dog from a child by a gulf as impassable as that which separates a stone from a dog. The superiority of human conduct is dependent on a complicated native repertoire of reactions, which is probably more significant in human life than precise native connections between definite stimuli and definite reactions.



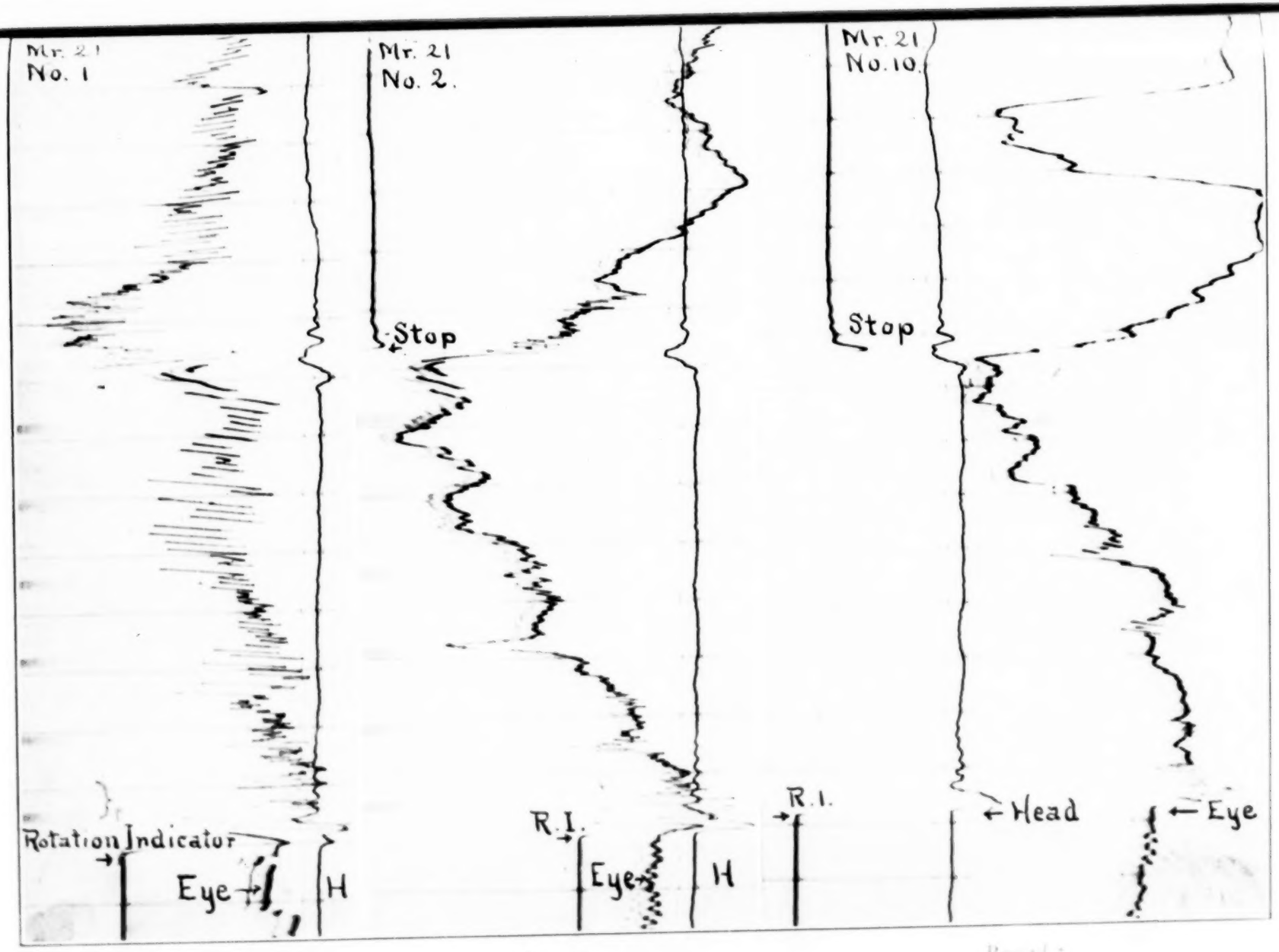
Record 1 Record 2 Record 3 Record 4

PLATE I. Changes in the character of compensatory eye-movements effected by protracted rotation.

Mr. 21

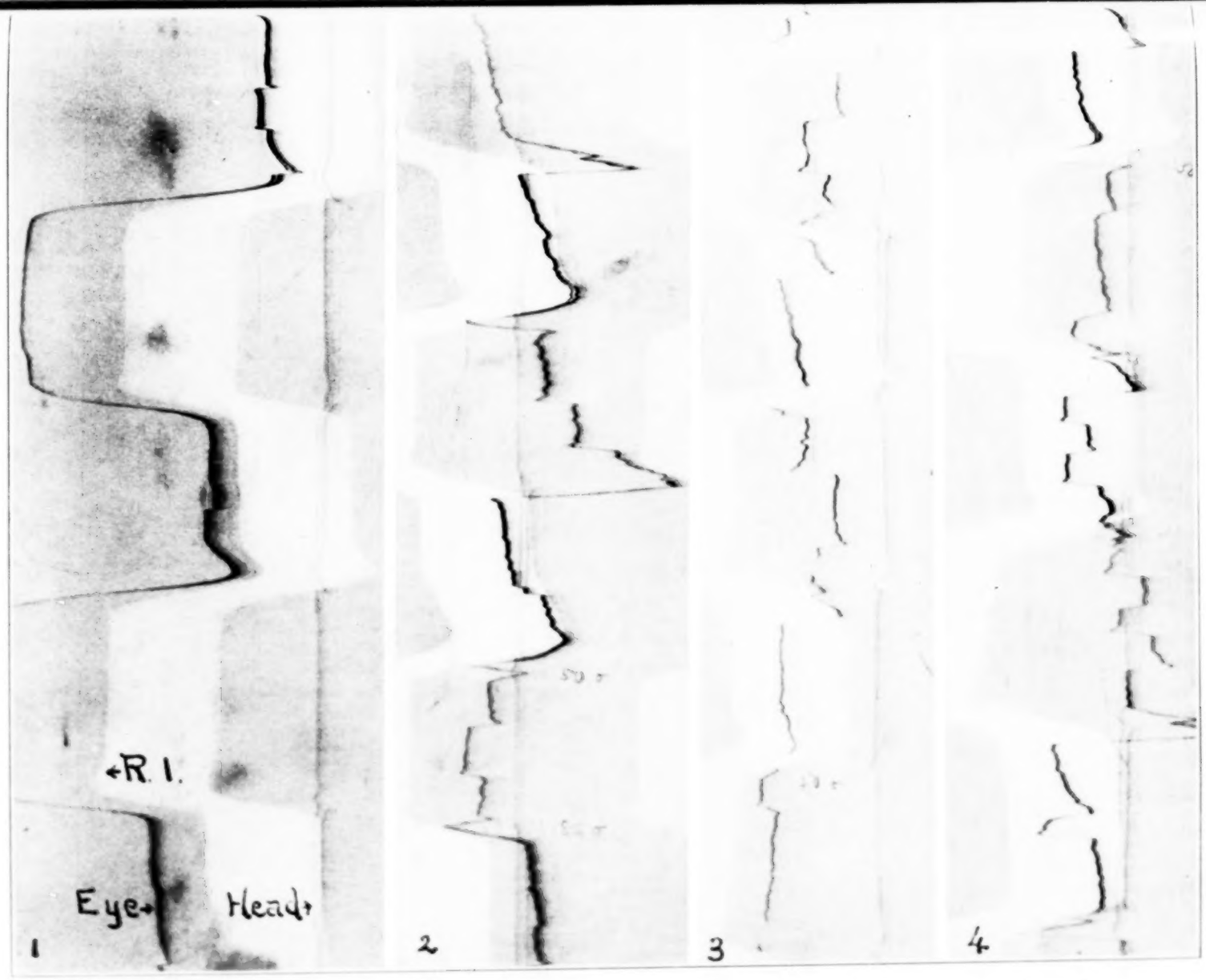
Mr. 21

Mr. 21



Record 1 Record 2 Record 3.

PLATE II. Changes in the character of vestibular nystagmus effected by repeated rotation.



Record 1 Record 2 Record 3 Record 4

PLATE III. Changes in coordinate compensatory eye-movements effected by closing the eyes.

DESCRIPTION OF PLATE I

In all records reproduced in Plate I., and in Plates II. and III. as well, the lower part of the record represents its beginning. That is to say, all records should be read from below upwards. The direction of eye-movement and of the rotation indicators are both reversed from left to right in these records.

Plate I., Record 1, is a record of eye- and head-movements during 20 complete rotations at a rate of approximately 100° a second. The line at the extreme left, marked 'rotation indicator,' shows the number of rotations and their approximate duration. The line 'Eye' is a record of the eye-movements during the nystagmus of rotation, the period of indifference and the subsequent nystagmus of stopping. The line marked 'Head' is a record of the inter-current head-movements. Time is shown by horizontal, light bands which are separated by two-second intervals.

Records 2, 3, and 4 are respectively the fourth, the eighth, and tenth records of a series of eye- and head-movements during oscillation. The rotation indicator returns to the record at the extreme of each oscillation and is marked O', O'', O''', O'''', for the corresponding oscillation. The eye-line shows very closely the change in compensatory nystagmus corresponding to each change in the direction of oscillation. It also shows the slow change in the direction of compensation just as the direction of oscillation changes. The head-line indicates that the head is relatively still during these oscillations. That is to say, no significant factor in the eye-line Record is attributable to head-movements. Record 3 corresponds in general to Record 2 but shows a slight reduction in the amplitude of eye-movement due to habituation. In Record 4, the nystagmus of compensation has degenerated into compensatory glides. Time is shown by dark, horizontal bands whose separation corresponds to one tenth of a second.

DESCRIPTION OF PLATE II

Records 1, 2, and 3 of Plate II. are respectively the first, second, and tenth records of the fourth day of the training series. They show the effects of repeated rotation on compensatory nystagmus. The eye-line, head-line and the rotation indicator are marked on all three records. The several rotations are shown by paired spots at the left of Records 1 and 2. They are eliminated from Record 3 in order to save space. The horizontal time-lines represent intervals of two seconds. These records show notable degeneration in both the nystagmus of rotation and the nystagmus of stopping (after-nystagmus).

DESCRIPTION OF PLATE III

Records 1, 2, 3, and 4 of Plate III. show the changing character of coördinate compensatory eye-movements in repeated series of voluntary oscillations of the head. In Record 1, the right eye was open. In Records 2, 3, and 4, both eyes were closed. In taking these records, the head-movement factor was eliminated from the record of the eye-movements by a new system of optical compensation. This compensation is indicated by the practically straight head-lines in all the records of Plate III. The extents of the voluntary head-movements are indicated by the oblique parts of the white, longitudinal bands. In these records, ten degrees of head-movement is represented by a displacement of twenty-six millimeters of the white band. Records 2, 3, and 4 show the rapid degeneration of coördinate compensatory eye-movements when both eyes are closed. The records immediately took the form of Record 1, when vision was reëstablished by opening the right eye.

A STUDY OF ESTHETIC JUDGMENTS

BY KATE GORDON

Introduction.—The most fundamental question of esthetics, and the most tantalizing, is the question of a standard. Are beautiful things really beautiful, or do we only think them so, because they give us a certain kind of pleasurable feeling, feeling which we have been taught to call 'disinterested,' 'immediate,' 'universal,' etc.? Is beauty constant? That is, are there any common external characteristics which beautiful objects must have in order to arouse such feelings? If there are none, then all men are equal in the value of their esthetic judgments, because then we have no objective facts with which to compare or verify their judgments. But if there are such external characteristics, they are the basis of a standard of beauty. The very existence of schools of art, and of rules of composition and of criticism seem to imply a belief in a standard. If the standard is there, how do we approach it to know it? These reflections led me to try to find out how well different persons would agree in their estimates of the relative beauty of the objects in a concrete series of color designs.

The Material Used.—For this purpose fifty colored plates of oriental rugs were used. All were printed on cards 4 by 6 inches in dimensions. The pictures were excellent reproductions of fine rugs. They appeared to afford a genuine esthetic experience to most of those persons who took part in the experiment. The rugs appeared to have a fairly wide range of esthetic values, though it is probable that the difference between 'best' and 'worst' represents a different emotional range for the different subjects.

Manner of Presentation.—The pictures were divided into two series in the following way. Three persons arranged the fifty pictures in an order of merit and their judgments were averaged. The pictures which stood first, third, fifth, etc.

were then called 'Series One,' and those which stood second, fourth, sixth, etc., were called 'Series Two,' thus making two series of 25 members each. This was done to see whether results obtained with Series One could be repeated with Series Two. The pictures of a given series were laid in chance order on a gray background, and to the person who was to judge them was handed a card of typewritten instructions as follows:

> Here are some pictures of Oriental Rugs. Please arrange them in the order of their beauty. At the extreme left put the one which seems to you to be the most beautiful, next to it the one which is second in beauty, etc., and at the extreme right the one which is least attractive. Do not try to think of rules or theories of art. Judge them by the pleasure or the displeasure they give to you. Take all the time you want, and make any rearrangements you wish.

Each person arranged the two series separately, and the order of choice was recorded by me.

The Subjects.—The number of persons who gave judgments was 207. All, except one negro, were Caucasians. About one half were undergraduate students in the Carnegie Institute of Technology. Among the others were a few graduate students, and members of the faculty and staff of the same institution. There were a few students of the University of Southern California. The rest were men and women with no academic connection. The latter were probably above the average in artistic training. Out of the whole group there were 20 who may be looked upon as 'experts.' I have counted as experts those whose professional duties demand some exercise of judgment in the matter of color. There were among the 20 some 11 persons engaged in art instruction as professors or teachers or supervisors, 3 rug dealers of more than local reputation, 3 painters, and 3 persons in professional connection with painting departments of museums.

Wide Diversities of Individual Judgments.—The first striking result is the great variation in opinion as to the relative merits of the rugs. Every rug, without exception, was rated very high by some persons and very low by others, that is, it was put within 3 places of the top and the bottom of the series respectively. Nearly every rug was put quite at

the top and at the bottom by different judges. The following distributions of choices will illustrate the variations.

TABLE I

DISTRIBUTIONS OF CHOICES ON RUGS

Place Given	Rug 1. No. of Persons	Rug 5. No. of Persons	Rug 8	Rug 17	Rug 22	Rug 25
1.....	24	6	29	2	0	1
2.....	25	6	12	4	2	3
3.....	24	8	12	4	2	1
4.....	21	16	11	3	3	1
5.....	11	18	6	6	4	3
6.....	20	15	4	5	2	2
7.....	14	12	6	6	7	2
8.....	9	10	3	3	11	5
9.....	4	12	8	12	1	1
10.....	6	8	9	10	5	3
11.....	7	8	4	14	7	6
12.....	2	7	7	9	7	8
13.....	6	11	1	10	8	2
14.....	3	6	5	9	12	5
15.....	5	7	7	15	7	5
16.....	4	10	7	13	13	3
17.....	4	3	6	15	17	12
18.....	3	7	3	10	18	6
19.....	1	9	4	13	12	11
20.....	3	5	5	14	12	7
21.....	1	6	8	9	14	12
22.....	2	5	8	4	10	16
23.....	1	3	14	8	9	14
24.....	0	2	13	2	10	29
25.....	0	1	9	1	7	43

In this table the first column at the left gives the places assigned in an order of merit to the rugs. The remaining six columns show the number of persons who assigned a given rug to such places. For example, Rug 1, which had the highest average position in this series, was given first place by 24 persons, second place by 25 persons, third place by 24 persons, etc., whereas Rug 25, which had the lowest average position, was assigned first place by 1 person and last place by 43 persons. Rug 8 is illustrative of a small group which showed bi-modal distributions. There were a few cases in which the distributions were similar to those of pure chance.

The distributions of choices in Series Two were closely similar to those of Series One in the several types. In spite of the wide spread of these placements there is after all a preponderance of choices in favor of certain rugs.

Several of the subjects felt that their judgments were likely to vary largely from day to day. It was possible to secure a second trial of the two series from 38 persons, the time interval between the trials being not less than 3 weeks. In every case the subjects said that they had by that time forgotten practically all of their previous judgments.

Stability of Individual Preferences.—The measure of a person's agreement with his own previous preferences was obtained by correlating the rank order of the rugs in the two trials, by the formula

$$r = 1 - \frac{6\Sigma D^2}{n(n^2 - 1)}.$$

For Series One the coefficients ranged from −.23 to +.94, with a mean of +.71 and standard deviation 18.5. For Series Two the range was −.17 to +.96, mean +.72, S.D. 15.7. It is clear from these figures that in some cases a person's second arrangement of the pictures had no significant correlation with his first. But these cases are exceptional, and the greater number showed a fair degree of self consistency.

Agreement of Individuals with Group Judgment.—When 101 persons had performed the task of arranging the pictures their various estimates were combined in an average. For example a certain rug would be given first place by one person, third place by another, twelfth place by another, etc. These numbers were added, and the rug which had the smallest resulting number was assigned first place in the composite order of merit. The rug which had the largest resulting number was called last or twenty-fifth in the composite order. Comparisons were made between this composite order and the individual arrangements of each member of the group, and also of another 106 persons who were not members of the first group. The above quoted correlation formula was used. Counting the two series this made 414 coefficients which were computed between individuals and the group order. The range of these is very great,—from −.12 to +.80 for Series One, and from −.32 to +.84 for Series Two. The respective means are +.42, S.D. 19.6, and +.41, S.D. 22.5.

The individual experts had lower correlations with the group than did the non-experts. Their coefficients for Series One ranged from −.10 to +.60, mean +.30, and for Series Two from −.32 to +.71, mean +.31.

A few inter-correlations were computed between pairs of experts selected at random, 12 such coefficients for each series. These ranged on the first series from −.26 to +.65, with a mean of +.20, and on the second series from −.29 to +.61, with a mean of +.19. In other words, the experts agreed very little with the group, and hardly at all among themselves.

The question here suggests itself whether agreement with the group is a constant quality or characteristic in the individual. That is to ask whether the persons who agree most closely with the group in judging one series of rugs will be the ones to agree most closely with the group in judging the other series. This was answered as follows. The correlations of individuals with the group for Series One were plotted against the correlations of the same individuals with the same group on Series Two. [See Table 2.]

Since the correlation coefficients were exact numbers the Pearson *r* was used, and the result was +.47, p.e. .036.

Agreement of Group with Group.—Five groups of subjects, 20 in each group, were compared. Groups I., II., III., and IV. were mutually exclusive groups selected at random. Group V. comprised the 20 experts mentioned above. The averaged order of merit for the rugs for the 20 persons of Group I. was correlated with the averaged order for the 20 persons of Group II. and so on. In this way 10 group intercorrelations were secured for each series. The mean intercorrelations of the 4 non-expert groups is +.76 (range +.61 −+.86) for Series One and +.84 (range +.79−+.94) for Series Two. The mean correlations of the expert group with the other groups is +.55 and +.53 respectively for the two series. Taking the five groups together and combining the series the mean intercorrelation is +.70.

Four mutually exclusive groups of 50 persons each were compared, the averaged order of rugs for each group being

TABLE 2

Correlation Coefficients for Series One

Correlation Coefficients for Series Two	−20	−10	0	10	20	30	40	50	60	70	80	90
90							1			3		
80							1	1	7	4	3	
70						3	3	4	4	3	2	
60					2	3	4	7	11	9	2	
50				1	2	5	11	8	8	2	1	1
40			4	1	1	5	11	7	8	1		
30	1			2	1	6	5	3	1	4		
20						2	1	3	4			
10	1				2	1	2	1	2			
0			1		2	1		1				
−10					1			1	1			
−20			2									
−30			1									
−40												

Scatter Diagram of Correlation Coefficients. Agreement with Series One Plotted Against Agreement with Series Two. $n = 207$, $r = +.47$, p.e. $= \pm .036$.

correlated with the averaged order for the other groups. The intercorrelations between the groups were, for Series One +.81, +.82, +.83, +.85, +.93, and +.96, and for Series Two +.84, +.87, +.87, +.90, +.90, and +.92. The mean is +.87 for one series and +.88 for the other.

A comparison was made between the average arrangement of the first 30 men tested, and the first 30 women. The coefficients were +.86 for Series One and +.86 for Series Two.

Finally the group arrangement for the first 100 persons was compared with the group arrangement of the second 100 persons. The result is a coefficient of +.93 for Series One, and of +.95 for Series Two. It appears then that as the numbers of judges included in the groups are increased the higher becomes the correlation between groups. In the case of the present data, groups of twenty agree to the extent of +.70, groups of fifty by +.87, groups of one hundred by +.94.

Summary of Conclusions

1. Every observation made with Series One was confirmed by the results of Series Two.

2. There was very wide diversity of individual judgments on the rugs, and this was even more marked in the case of experts than of others.

3. The majority of persons who judged the pictures twice were fairly consistent with themselves, mean coefficients were +.71 and +.72.

4. The correlation of individuals with groups tended to be positive, though not high, mean coefficients being +.41 and +.42.

5. Those who tended to reflect group judgment in the one series tended also to do it in the other series. Pearson $r = +.47$.

6. No large negative correlations were obtained.

7. Between group and group the measure of agreement is large and it increases with the size of the groups.

8. The experts who, as individuals, had mean correlations of +.19 and +.20 with a large group, had, as a group, mean correlations of +.53, +.55 with other groups.

9. It may be held that although the large groups agree on a certain order of merit for these rugs it may yet be a "wrong" order. I shall be willing to believe this when it can be shown that experts themselves, judging under experimental conditions, can achieve so high a degree of stability as the large groups here show.

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A COMPARISON OF THE SCALE OF VALUES METHOD WITH THE ORDER-OF-MERIT METHOD

BY EDMUND S. CONKLIN AND JOHN W. SUTHERLAND

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The idea of a scale of values is not new, but the method has been little developed in spite of the fact that it seems to have certain very definite advantages. Henmon in 1911 [1] reported a simple scale of five degrees of confidence. The next year Strong[2] published a study in which a still shorter scale of three steps was utilized. Hollingworth has made use of scales of four and of five steps concerning certainty and concerning persuasiveness.[3] The purpose of the present study was to test the scale of values as a method for psychological research and to compare it with the order-of-merit method, its nearest relative.

For the comparison study Hollingworth's judgments of the comic was selected.[4] His procedure was repeated exactly, with the exception that we used forty jokes instead of his thirty-nine. Each of ten subjects once a week for five weeks arranged these jokes in their order of merit by his group modification of the original order-of-merit method, that is, the distribution of the jokes into ten piles, each pile representing a degree of merit although that degree of merit remained undefined and undescribed.

Simultaneously with this repetition of Hollingworth's study the same material, the forty jokes, was used on ten other subjects once a week for five weeks by the scale-of-

[1] Henmon, V. A. C., 'The Relation of the Time of a Judgment to its Accuracy,' *Psychol. Rev.*, 1911, 18, 186–201.

[2] Strong, E. K., 'The Effect of Length of Series upon Recognition Memory,' *Psychol. Rev.*, 1912, 19, 447–462.

[3] Hollingworth, H. L., 'Judgments of Similarity and Difference,' *Psychol. Rev.*, 1913, 20, 271–289 and also 'Judgments of Persuasiveness,' *Psychol. Rev.*, 1911, 18, 234–256.

[4] 'Judgments of the Comic,' *Psychol. Rev.*, 1911, 18, 132–156.

values method. Because Hollingworth had used ten piles or groups in his procedure it was necessary to devise a scale of ten steps, as follows:

1. Very best joke possible.
2. Excellent joke.
3. Very, very good joke.
4. Very good joke.
5. Good joke.
6. Funny, not a good joke.
7. Very absurd, almost funny.
8. More than slightly absurd.
9. Just slightly absurd.
10. Dull, stupid, not even absurd.

In justice to the possibilities of the scale-of-values method it should be pointed out that the above scale is perhaps too complex and attempts to care for too many grades. An analysis of the scale shows that there can be no question about the difference between the first two grades. The interrelation of phrasing functions one way only. The very best joke possible is of course an excellent joke, but the term 'excellent' does not signify the limits of possibility. The distinction between the second and third grades was found very satisfactory. The effect of emphasis was employed in relating the third and fourth steps. A few subjects naturally objected to the fineness of discrimination here distinguished. But that the phraseology suggested a real difference none would disagree. The word funny in the sixth grade marks a doubtful descent from the fifth position, but the addition of the phrase 'not a good joke' served very well to make clear a distinction. The introduction of this word was suggested by the word 'absurd,' the latter proving conclusively its value in the lower steps of the scale. Simple as these words appear, more subtly suggestive terms could not easily be found to stand the repeated experimental testing to which the scale was subjected. It was found that the term 'absurd' has a very general connotation; it describes something which perhaps may not have the power to induce even an incipient tendency to smile. The 'funny' experience has usually a

decidedly emotional tone, demanding explicit response on the part of the average subject. Disclaiming any suggestion of perfection in the scale and admitting its limitations both in phraseology and grading consistency the results of its repeated application indicate that it represents one of the most practical selections possible under the conditions externally imposed. In but very few instances did any doubt exist as to the import of the phraseology employed. It is indeed hard not to appreciate the metaphysical implications of such a phrase as 'the very best possible.' It immediately raises the question as to the significance of philosophical ultimates. Some subjects even expressed themselves as unable to conceive of such a descriptive adjective as the 'very best possible'; while two or three others had never yet heard the best joke possible, and they were equally positive that they never would hear it. But these recalcitrants readily descended from such an attitude and were willing to agree that there can be no doubt but that every normal individual has, at some time or other, reacted whole-heartedly to some joke which seemed to eclipse all previous like experiences in its degree of emotional appeal. The fact was very generally appreciated that it was in the light of such experiences that the above phrase had been employed. Such a phrase takes cognizance of the fact that it is possible for the individual to have an emotional experience in the presence of the ludicrous which, for the time being, so monopolizes his mental and bodily functions as to evoke the admission that the joke is one of the very best possible.

Twenty subjects were used for the study, ten, five men and five women, for the group order-of-merit procedure and ten, seven men and three women, for the scale-of-values method. Five of the twenty were members of the psychology department staff. The remainder were undergraduate and first-year graduate students. All experimental sittings were individual so that the experimenter could give undivided attention to each subject experimented with. Objective conditions were maintained as uniform as possible; most of the sittings were in the psychological laboratory. No suggestions or distractions whatever were allowed to influence the inde-

pendent judgments of the subjects. Following the procedure of Hollingworth each observer was given five trials at intervals of not less than a week. The order of the cards at any one sitting was always the reverse of the order used at the preceding sitting for that individual subject. The order was controlled by numbers on the back of each card which remained unknown to the subjects.

Instructions given to the subjects in the group order-of-merit experiment were the same as used by Hollingworth. Those used for the scale-of-values subjects were as follows:

1. A number of comic selections or jokes is provided and these are to be arranged according to the values indicated on the Scale of Values before you.
2. Read the Scale of Values very carefully.
3. Be sure that you understand every grade in the scale. If you have any doubt whatever about the meanings of the words, such meanings will be gladly explained.
4. If you think that a particular joke is of the very best possible grade, you would grade it as the 'Very Best Joke Possible.' But if the joke is not the very best possible, it may, nevertheless, be an excellent joke, and so would be designated as an 'Excellent Joke.' And so on down the scale, until finally you have what you believe to be jokes which are 'Dull, Stupid, Not Even Absurd.'
5. Take plenty of time to read the jokes; see the 'point' in each joke, if there is such; and then carefully compare with the Scale of Values.
6. When you have done so report your grade to me.

It will be noted that by the scale-of-values method the subject was given but one opportunity at each sitting to classify each joke in terms of some one grade indicated on the scale. This means that he was permitted to handle the same card but once during each sitting. After reading the joke carefully and reacting to its content, he was given unlimited time in which to refer to the scale, to review once again the different grades possible, and to compare the joke now being judged with the grades set before him. In the group order-of-merit method the procedure is significantly different. No restrictions were imposed. The subject could shift the cards from pile to pile as he thought best to do, he could revise his judgment at pleasure.

While the experiment naturally aroused the curiosity and interest of most of the subjects, especially with regard to the qualities of their judgments from the standpoint of self consistency and comparative grading, no personal communi-

cations regarding likes and dislikes were known to have been exchanged during the time occupied by the experiment. It was almost impossible for the subjects to remember many of the jokes, while the positions assigned them were still harder to recall.

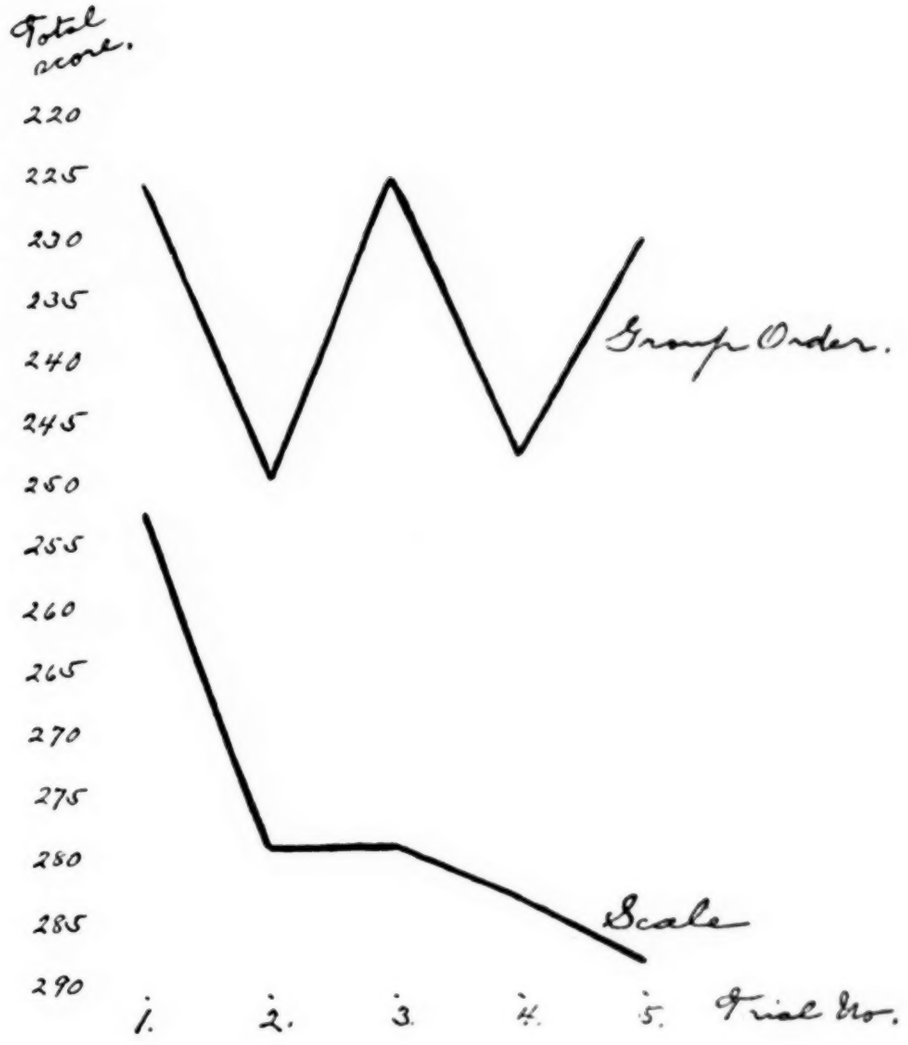
The forty jokes used were selected from a preliminary collection of over four hundred. They were typed on cards three inches by five. Each card was numbered on the back for the convenience of the experimenter in recording and for control in arranging the order of the cards.[1]

The tabulated data of all the sittings was summarized by obtaining the average standing of each joke in each trial or set of sittings for each of the two methods, and also the mean variation for each average. The sum of the joke averages in each trial forms an excellent basis for comparison. The following table presents these results:

Trial No.	Scale Av. Total	Group Av. Total	Scale Av. M.V.	Group Av. M.V.	Difference between Scale and Group Av. M.V.
I.....	251.5	225.2	1.94	2.77	.83
II.....	278.7	248.9	2.10	2.84	.74
III.....	278.4	224.3	2.02	2.60	.58
IV.....	282.2	246.8	2.23	2.34	.11
V.....	287.4	229.5	2.14	2.32	.18

It will be noticed in the table that the scale totals are constantly larger than the group-order totals indicating that the lower steps in the scale were more frequently used. The steady increase in these totals as compared with the fluctuating totals obtained from the group order method is represented on the accompanying graph and will be discussed below. It is remarkable too that in no case does the mean variation in the group-order method become less than that with the scale, and is usually much larger.

[1] The actual jokes used are not reproduced here for economy of space. They have been filed with the librarian of the University of Oregon from whom they may be borrowed.



The following comparisons are through correlations obtained by the method of rank differences:

CORRELATIONS BETWEEN TRIALS

	Scale	Group
Trials I. and II.	.82	.70
" I. and III.	.85	.80
" I. and IV.	.79	.70
" I. and V.	.69	.73

CORRELATION BETWEEN POSITIONS OF THE JOKES IN BOTH METHODS

Between Trials I. in scale and group	.46
" " II. " " " "	.49
" " III. " " " "	.47
" " IV. " " " "	.58
" " V. " " " "	.41

The correlation between the two final orders of the cards in the two methods is .55. The following table presents the final order:

FINAL ORDER OF MERIT OF THE JOKES AS DETERMINED BY FIVE TRIALS BY BOTH SCALE-OF-VALUES AND GROUP ORDER-OF-MERIT METHODS

Joke No.	Scale Av. Score	Group Av. Score	Rank in Scale Order	Rank in Group Order
1	39.2	24.5	34	6
2	26.7	23.1	2	2
3	40.0	29.8	36	19.5
4	31.0	25.4	11	9
5	37.3	31.1	28	24
6	31.4	32.1	12.5	26
7	36.5	33.6	25	28
8	40.2	36.4	37	35
9	40.8	30.9	39	23
10	36.1	26.4	23.5	11
11	39.3	38.8	35	39
12	33.8	23.6	17.5	4
13	25.7	27.9	1	13
14	34.6	29.9	20	21
15	37.7	34.2	29.5	29.5
16	40.6	37.8	38	38
17	28.3	28.5	4	16.5
18	30.2	23.1	7.5	2
19	37.8	35.9	31	33
20	35.3	26.1	21	10
21	30.3	34.2	9	29.5
22	33.7	28.4	16	15
23	29.6	24.1	6	5
24	37.7	28.5	29.5	16.5
25	38.2	33.3	32	27
26	31.4	31.9	12.5	25
27	36.9	35.2	26	31
28	36.1	39.4	23.5	40
29	30.7	24.9	10	7
30	41.0	36.0	40	34
31	38.3	30.5	33	22
32	33.5	28.0	15	14
33	28.6	27.2	5	12
34	31.9	35.3	14	32
35	27.7	23.1	3	2
36	30.2	25.1	7.5	8
37	35.4	28.9	22	18
38	37.0	36.9	27	36
39	34.0	29.8	19	19.5
40	33.8	37.7	17.5	37

A complete set of all tables made in this study has been filed with the librarian of the University of Oregon from whom they may be borrowed.

The correlation of .55 between the final orders obtained by the two experimental methods, and also the significant

correlations between corresponding trials by the two methods indicates a fairly high degree of community, and should give the scale-of-values method respectable status. The fact that it is no higher challenges consideration. There is a conspicuous difference in the procedure due to a difference in the instructions used. Reference to them will reveal that the scale-of-values method demands a descriptive response concerning the subject's reaction to the given joke presented in terms of the scale which is constantly before him; this reaction to be given entirely without consideration of or comparison with other jokes. And the same scale is used for the description of the reaction to every other joke presented. Whereas in the group order-of-merit method the subject is permitted to revise his judgments indefinitely. This means that he constantly changes his cards from pile to pile at varying intervals, so that ultimately his decisions come to be based on what may be purely cognitive processes and not in any way upon the immediate affective response which constitutes the core of the reaction to the ludicrous. When the first affective response has passed, the value of the joke may decrease markedly. This may take place unconsciously due to the inhibitive effect of other perceptive and imaginal factors, the appearance and reaction to jokes of a very different type, for example. Contrarily an exaggerated position may be assigned a joke for the reason that meanwhile the subject has read other jokes which distort the judgment now being rendered. This defect, if it is a defect, has obviously a dual aspect. It may function either way, eventually resulting in a comparatively small variation which is no index to the validity of the method. In a great many cases, however, it cannot be said that the first judgment made by a subject counts very much if at all, while the precise significance of subsequent shiftings has not been determined. This procedure is certainly calculated to nullify a true evaluation of the joke as it first affects the subject, at which time he may give the joke a relatively high or low grading only subsequently to take the card out of its original place because it later appears to be better or worse than some other joke upon which judgment has just been passed.

This theoretical analysis is confirmed by observing the course of the score totals from trial to trial. Except for the insignificant drop of three tenths of a point in the third as compared with the second trial these total scores showed a steady increase from trial to trial ending wth a score total 35.9 greater than the first trial total, in the scale-of-values method. This means undoubtedly that the immediate responses to the jokes time after time was to place them lower in the scale, that is the tendency for the whole group was downward on the scale. The figures for the group order-of-merit method show a very different course. Here there is a marked oscillation[1] with the third total less than the first and the final total only 4.3 greater than the first trial total. The course of the two curves being so different there must be some decidedly different process functioning in the production of one which is not present in the other The most obvious difference is that in the directions given to the subjects. Apparently the scale-of-values method records an immediate response which is primarily feeling determined by unknown and probably unconscious tendencies; while the group order-of-merit method reports a reaction far less governed by feeling than by a thoughtful consideration of the relative merits of the jokes as jokes. The one is a direct response, the other involves the weighing of evidence.

The constantly greater variability as indicated by the M.V. for each trial of the group order-of-merit method would be expected in part from the method itself. The insistence of the method upon ten piles every trial even though there be but one card in some of the piles would artificially force some of the jokes up if others went down, thus raising the variability, or artifically retain at an abnormally high grade certain of the jokes. The constantly higher scores found in the group order-of-merit results are doubtless also due to the same item of procedure. In his paper Hollingworth suggests an exceedingly valuable measurement, the ratio of the mean varia-

[1] Hollingworth's trial totals showed a continuous decline more like our scale-of-values results, although without so great a fall. Why we should have obtained the variation from trial to trial when he did not can only be answered after further experimentation.

tion to the total number of positions used (M.V./P.). Translating our measures of variation into this measure we get the following table:

	Scale	Group Order
Trial 1	.19	.27
" 2	.21	.28
" 3	.20	.26
" 4	.22	.23
" 5	.21	.23

It is obvious that the M.V./P. values are far apart at the beginning of the series and approach each other through the series of trials until they are practically the same. Observation of the subjects undergoing the experiment and the difference in the methods used would indicate that the difference between the M.V./P. values is due to the degree to which primitive or personal feeling enters into the process. The directions and the procedure, especially the permission to arrange and reëvaluate, in the group order-of-merit method would, as has already been pointed out, eliminate to a considerable degree the personal feeling factor and leave the process a more purely impersonal and unemotional one. However, as the series of sittings continued the subjects became more and more bored with the task (we found it so and Hollingworth mentions it) which would make the subject less careful of his evaluations, more nearly approaching the snap judgment stage, which would mean that personal feeling was an increasingly influential factor. On the other hand the scale method was a far less tiresome task, it did not require the constant comparisons, the keeping of many jokes in mind, the rearrangements, and study to get exactly the ten piles required. If the instructions were followed the judgment was to be prompt and definitely personal, without consideration of anything but the subject's personal immediate reaction to the one particular joke before him. Primitive personal feeling could enter the process as freely as it might; it was expected, desired. The M.V./P. values by the scale method in beginning far below those of the group order of merit, conform to the interpretation just given. That they rise slightly

through the series would in terms of this interpretation be due to the gradual entering in of more logical interpretation or evaluation and a slight decline of the personal-feeling factor. This might very likely have been the case as the jokes grew stale and less able to arouse emotional reactions.

The difference just mentioned might be tentatively expressed in the form of a law: Where other things are equal the M.V./P. value varies inversely as the degree to which feeling or emotion influences the judgment. Reference to other studies of judgment, all by the order-of-merit method, tend to confirm this law. Strong[1] gives figures for the judgments of the relative persuasiveness of four different kinds of advertisements, ten advertisements in each set, twenty subjects. Translating his figures into the M.V./P. value gives the following:

Vacuum Cleaner Adv.	M.V./P. .24
Piano Advs.	M.V./P. .24
Breakfast Food Advs.	M.V./P. .22
Toilet Soap Advs.	M.V./P. .17

That toilet soap advertisements would arouse more personal feeling in the judgment than vacuum cleaner advertisements (this study was made in the early days of the vacuum cleaner) would seem to be obvious. This is supported by examination of the advertisements used. Four of the ten soap advertisements present babies conspicuously, others concern health and beauty, and all of the subjects were women. Wells's study of literary merit[2] presents the same thing. In the following table we have translated his P.E. figures into M.V. figures by the usual formula before calculating the M.V./P. value:

10 of Poe's tales	M.V./P. .036
10 American authors	
Clearness	M.V./P. .048
Force	M.V./P. .038
Euphony	M.V./P. .036
General merit	M.V./P. .035

[1] Strong, E. K., 'The Relative Merit of Advertisements. A Psychological and Statistical Study,' *Archives of Psych.*, No. 17, N. Y., Science Press, 1911, pp. 81.

[2] Wells, F. L., 'A Statistical Study of Literary Merit,' *Archives of Psych.*, No. 7, N. Y., Science Press, 1907, pp. 30.

The M.V./P. values are here all much smaller than the others quoted, undoubtedly due to the nature of the material used and the training of the subjects. The judgments of Poe's tales were made by forty female undergraduate students in English, and the judgments of American authors were made by graduate students in English. But the tendency of the figures is to conform to the suggested law. Clearness is a matter of cognitive judgment by rhetorical rules; force is also a rhetorical matter, but more likely to involve feeling response; euphony is still more likely to involve feeling; and so general and vague a thing as 'general merit' must inevitably be in terms of personal feeling even by technically trained graduate students in English. The judgment of Poe's tales by a group of women students, even though students of English, might be expected to have considerable of the emotional factor involved; and the emotional influence is indicated by the considerably lower M.V./P. value, lower in comparison with the other judgments in that group. We have here not reproduced all the items in Wells's list but the others conform to the suggested law. Sumner's study[1] of twenty-five belief propositions may also be cited. He complains in that study of the difficulty he encountered in getting his subjects to judge in terms of their 'subjective feeling of sureness' rather than upon the 'logical certainty' of the situation. Certainly the material he used would make this difficulty probable. Psychologists, however, trained to many kinds of experimental procedures, ought to be better able than the untrained to make this concentration and produce judgments more certainly in terms of their 'subjective feeling of sureness.' The figures support this as follows:

Psychologists tested (29)	M.V./P. .14
Non-psychologists (71)	M.V./P. .16

Again we find that where there is a preponderance of feeling in the judgment the M.V./P. value is the smaller. Wells's study of preference for picture postcards[2] as refigured by

[1] Sumner, F. B., 'A Statistical Study of Belief,' *Psychol. Rev.*, 1898, 5, 616–632.

[2] Wells, F. L., 'On the Variability of Individual Judgment,' Essays Philos. and Psych. in Honor of Wm. James, N. Y., 1908.

Hollingworth[1] gives an M.V./P. value of .201. The material used is comparable to the studies with advertisements and with jokes, and it will be seen that the M.V./P. for postcard preference is about the same and shows a comparatively high degree of influence by personal feeling.

This explanation for the differences found in the M.V./P. values is supplementary to Hollingworth's statement that "the size of this ratio M.V./P. would become smaller as the material came to be selected so as to disclose more pronounced or more objectively measurable differences."[1] Take any set of experimental material, jokes or postcards or advertisement or what not, and if within the set the numbers are conspicuously different from each other, in funniness or beauty, or appeal or what not, then of course the variability would be low, diminishing as the material became more and more discrete. But with this law of Hollingworth's alone we should be led into sore difficulties. We should be obliged to say that ten American authors (M.V./P., .035) or ten of Poe's tales (M.V./P., .036) was material containing more pronounced or more objectively measurable differences than a series of weights (M.V./P., .141). It would furthermore be necessary to conclude, if we used Hollingworth's interpretation alone, that the change of the M.V./P. value in a succession of sittings indicated a change in the objective measurability of the material, when the material of course had not changed. The interpretation here offered removes these difficulties and conforms to the long-observed fact that mankind is more alike in his fundamental feelings than in his non-affective intellectual behavior. Of course much more experimentation is necessary for its complete establishment.

Hollingworth's procedure for the classification of the jokes and their distribution into waxing, waning, and static types was followed in the treatment of our data with results which did not support his conclusions. Perhaps this failure to confirm is due in large part to the difficulties confronted in the definition of the different classes or kinds of jokes, and also to the uncertain definition of waxing, waning and static. It is

[1] 'Judgments of the Comic,' p. 145.

very difficult at times to know into what class a joke should be placed; or whether to call it waxing or static, or likewise whether to call it waning or static. We certainly did not find the tendency which Hollingworth found for the general rise of the waning and fall of the waxing cards on the last day of the series. We did find, however, in comparing the results of the two methods that the scale method and the group-order method agreed seventeen times out of the possible forty. This corresponds to the correlations and is again an indication doubtless of the degree to which the same mental processes functioned in the production of the results by the two methods.

A comparison of the correlations between trials indicates the same thing. Reference to the table above will reveal that the correlations between the first trial and each succeeding trial by the group order-of-merit method are practically constant throughout the series; while with the scale method there is a drop from .82 between the first and second trials to .69 between the first and fifth trials. Apparently in the group order-of-merit method the subjects work in a less personal manner treating the jokes on their merits as jokes and with the personal reaction or feeling response largely eliminated; while the scale method records the personal reaction primarily and consequently the changes from time to time as the personal reaction changes.

The indication of this study is to the effect that the scale-of-values method can be used for approximately the same purposes as the order-of-merit method, but that the scale-of-values method is a better means of obtaining a record of judgments or reactions influenced by the immediate emotional response to the situation.

PERFORMANCE TESTS FOR THREE-, FOUR-, AND FIVE-YEAR-OLD CHILDREN

BY HELEN T. WOOLLEY, PH.D.; AND ELIZABETH CLEVELAND

INTRODUCTION

The nursery school, established as part of the work of the Merrill-Palmer School, has an enrollment of about thirty-five children under five years of age. Part of the plan is to make as complete a study as possible of each child enrolled. Every child is given a careful medical examination and a mental examination. His family history and social background is studied, and notes kept of types of reaction. In undertaking to make mental examinations of children between two and five, we were confronted with the well-known difficulty of a dearth of standardized tests. The Stanford Revision of the Binet Scale was given to each child, and we then began to look about us for suggestions of other tests which might be used. A few preliminary trials with pieces of the Montessori apparatus had convinced us that it was too easy for our older children to have much value as additional experience or training, but might be used as a performance test, if standards of measurement were worked out. Accordingly, we selected the pink tower, and the three sets of cylinders as test material, and kept those pieces of apparatus out of the nursery until after all the children of three and four years had been examined.

The number thus obtained—27—was too few to serve as the basis of a scale of measurement. Though the children of our nursery school come from a wide variety of cultural and economic backgrounds, nevertheless they are, as measured by the Stanford Revision, a somewhat superior set. Of the 27 used in the standardizing of these tests, two had quotients below 90, five had quotients between 90 and 110, and the remaining 20 had quotients above 110.

Our next step was to send for children on the waiting list of the school. Seventeen of them responded. Of these fifteen were also given the Stanford Revision. Two tests were questioned because of the timid or antagonistic attitude of the child. Of the remaining thirteen, eight had intelligence quotients ranging from 100 to 110, and five had quotients above 110. Though the children on the waiting list were not as superior as those of the school, we were convinced that the group thus far tested was as a whole above the average of the community. Our next step, therefore, was to turn to the children of the day nurseries of the city. In the three nurseries we found twenty-five children of three and four years. It was not possible to give these children the tests of the Stanford series. After exhausting the day nurseries we were still thirty-one children short of the one hundred cases desired. These were obtained by asking the older children of two of our public schools in average districts of the city to bring their little brothers and sisters for the tests.

When our examinations of three- and four-year-old children were complete, we found that part of the tests were too hard for the four-year-olds and therefore wished to add a set of five-year-olds, hoping thus to obtain a workable set of norms for four- and five-year-old children. The five-year-olds were taken in part from a kindergarten in a superior residential district, and in part from kindergartens in the poorer and crowded neighborhoods. Children with the handicap of a foreign language were not included.

It is, of course, impossible to say that the fifty children of each age selected for these tests constituted a fair sampling of the community. We can only say that we adopted in each case a method of selection which promised to include representatives of all grades of normal children.

In classifying children by age, all those between the third and the fourth birthday were considered three years old, all those between the fourth and the fifth birthday four years old, etc. The exact chronological ages of the children are presented in the following table:

AGE IN YEARS AND MONTH OF CHILDREN TESTED

Years	3	4	5
Months			
0	1	7	1
1	4	3	2
2	7	9	2
3	2	3	10
4	0	1	6
5	10	6	9
6	6	6	9
7	6	1	5
8	3	2	1
9	2	6	1
10	3	2	3
11	6	4	1
Median	3^6	4^5	5^5

The median age of the three-year-olds was three years, six months; that of the four-year-olds four years, five months; and that of the five-year-olds five years, five months. All of the testing—both the Stanford Binet tests and the performance tests—were given by Miss Cleveland under Mrs. Woolley's direction.

PERFORMANCE TESTS WITH MONTESSORI APPARATUS

(Given to 50 three-year-old, 50 four-year-old, and 50 five-year-old children, chosen as follows.)

Three and Four Years Old

From Merrill-Palmer Nursery School	27	
From Waiting List of Merrill-Palmer Nursery School	17	
From Sophie Wright Day Nursery	6	
From Tau Beta House Day Nursery	9	
From Neighborhood House Day Nursery	10	
Brought by Public School pupils	31	100
Five Years Old		
From three public school kindergartens		50
		150

DESCRIPTION OF TESTS

I. *Pink Tower*

Material and Purpose.—The material used was the Montessori Pink Tower, consisting of ten blocks to be placed one on another in order of size, from a ten-centimeter cube to a one-centimeter cube, the purpose being to determine the time required for a perfect performance.

Method.—The child was taken into a room where the test could be carried on without interruption. In case of shyness some one familiar to the child (his teacher or the person who brought him) was permitted to be present, though warned to keep in the background and refrain from interference or comment.

The tower, properly built up on the floor, was exhibited, and the following formula used:

> Look at this pretty pink tower. Do you see how it is built with the biggest block at the bottom and then the next biggest, and so on up to this little baby block on the top? Do you think, if I knock the tower down, that you could build it again just the same way, with the big block on the bottom and the little block on the top?

Whatever the response, the tower was then knocked down, care being taken to have the blocks near together in easy reach, and thoroughly mixed. The child was then told "Now, go ahead," and allowed to build the tower without assistance or comment other than "Good," or a nod or smile if he seemed to need encouragement. If the blocks were incorrectly placed when he had finished, he was asked, "Does that look right to you?" The question was asked after the time had been recorded. In every case the child professed himself satisfied with the result and this chance to correct mistakes might as well have been omitted.

Recording.—Success or failure and time in seconds were recorded. A three-minute time limit was allowed and only a perfect performance counted as success. The blocks were numbered in order of size, from 1 to 10, and the order in which they were placed was recorded.

II. *Cylinders*

Material and Purpose.—The material used was the Montessori cylinders, to be fitted into corresponding holes in wooden frames. There are three sets of ten cylinders each, the first varying in diameter, the second in both diameter and height, and the third in height only. The purpose in each case was to determine the time required for a perfect performance.

Method.—The cylinders of the first set were taken out

one by one and placed beside the frame on a low bench or chair so that the child could look down into the holes. They were then returned one by one to their holes, and the following formula was used:

> See how these cylinders come out of their holes. Each has a hole where it just exactly fits, the big ones in the big holes and the little ones in the little holes. Now I am going to take them out again and mix them all up and see if you can put them back into the holes where they just fit.

The cylinders were then removed and placed again beside the frame, thoroughly mixed, and the child told "Now, go ahead," and allowed to proceed as in the previous test. With the second and third sets the first illustrative replacing was omitted.

Recording.—The time required to fit the cylinders into the holes was recorded in seconds; a three-minute time limit was allowed, and only a perfect performance counted as success. The sets were lettered *A*, *B*, and *C*, and the cylinders numbered according to size from 1 to 10, and the order of placing recorded.

Results

The results of the tests are presented in two forms. Table I. gives a detailed report of the number of successes at each age in each test, and the times of all the successful performances. Table II. sums up the content of Table I. in the form of ten-percentile scales for each test at each age, three, four and five years.

Discussion of Results

Conclusions.—The definite relation between age and performance apparent in the results of these tests indicates that they measure satisfactorily the abilities of four- and five-year-old children, but select only superior three-year-olds (see Table I. and Table II.). For the three-year-old children the second set of cylinders is easier than the first, due probably to the effect of practice. The third set of cylinders is the hardest for all, and the pink tower comes between the second and third sets of cylinders in difficulty. Most of the trouble with the pink tower comes with the correct placing of the larger blocks. Only 14 of the three-year-olds failed with the

last five blocks, so that the use of these blocks alone should make a satisfactory performance test for them.

The time required by most of the children for a satisfactory

TABLE I

TIME OF SUCCESSFUL PERFORMANCE IN SECONDS

Pink Tower			Cylinders									
			A			*B*			*C*			
3	4	5	3	4	5	3	4	5	3	4	5	
No. T.	No. T.	No. T.	No. T.	No. T.	No. T.	No. T.	No. T.	No. T.	No. T.	No. T.	No. T.	
15 19	24 19	40 19	27 27	44 25	50 18	32 35	43 32	49 24	13 32	19 34	36 34	
31	25	23	31	28	24	35	32	26	40	35	36	
34	29	24	31	32	27	37	35	28	55	39	40	
35	30	26	34	35	31	40	36	28	56	40	41	
36	30	29	35	35	31	40	37	29	61	45	43	
36	31	29	36	36	35	40	40	31	64	47	43	
40	34	29	37	37	35	42	43	33	71	47	44	
40	34	29	40	38	37	42	43	33	74	49	44	
45	36	30	40	39	37	45	44	35	84	49	45	
60	36	30	42	40	37	45	44	35	90	55	46	
65	37	31	43	41	38	45	44	35	110	60	47	
69	39	31	44	41	39	50	44	35	114	60	48	
70	39	31	47	42	39	51	44	35	135	61	49	
80	43	32	50	45	39	53	46	36		67	53	
90	45	32	51	45	39	54	48	36		75	56	
	47	32	52	47	41	55	49	36		89	59	
	47	32	60	49	41	55	50	37		120	59	
	50	32	60	50	42	56	50	38		121	59	
	54	32	61	50	44	61	50	39		179	59	
	61	33	61	50	44	68	52	40			61	
	61	33	65	50	45	70	54	40			61	
	80	34	68	50	46	71	55	40			67	
	83	36	77	52	46	75	57	41			71	
	101	36	86	61	46	75	60	41			74	
		37	90	64	47	76	60	41			77	
		39	95	64	47	80	60	42			80	
		40	143	65	47	110	61	43			80	
		41		65	47	115	61	44			81	
		42		67	48	116	61	44			82	
		43		70	48	160	61	44			89	
		44		77	51	165	61	44			96	
		44		80	51	178	62	45			119	
		45		80	53		65	45			120	
		45		80	54		71	46			140	
		48		87	54		75	46			156	

performance indicates that a three-minute limit is an ample time allowance. There were only eight of the 150 children who attained a success after two minutes and a half had elapsed.

TABLE I (*continued*)

Pink Tower			Cylinders								
			A			*B*			*C*		
3	4	5	3	4	5	3	4	5	3	4	5
No. T.	No. T.	No. T.	No. T.	No. T.	No. T.	No. T.	No. T.	No. T.	No. T.	No. T.	No. T.
		50		89	54		77	51			169
		55		90	56		80	53			
		66		91	57		85	55			
		81		91	57		95	61			
		96		94	60		108	63			
				101	61		115	64			
				119	61		124	66			
				123	62		151	68			
				163	63			70			
					80			80			
					89			84			
					96			89			
					98			125			
					115			143			
					148						

TABLE II

TEN-PERCENTILE TABLES. TIME IN SECONDS

Based on the Examination of 50 Children of Each Age in Each Test

	Pink Tower			Cylinders								
				A			*B*			*C*		
	3	4	5	3	4	5	3	4	5	3	4	5
10...	Inc.	Inc.	Inc.	Inc.	Inc.	80	Inc.	Inc.	80	Inc.	Inc.	Inc.
20...	Inc.	Inc.	96	Inc.	94	60	Inc.	108	63	Inc.	Inc.	Inc.
30...	Inc.	Inc.	48	Inc.	87	54	Inc.	75	46	Inc.	Inc.	156
40...	Inc.	Inc.	43	Inc.	70	48	160	61	44	Inc.	Inc.	89
50...	Inc.	Inc.	37	90	64	47	76	60	41	Inc.	Inc.	77
60...	Inc.	61	33	61	50	44	68	52	40	Inc.	Inc.	61
70...	90	45	32	51	45	39	54	48	36	Inc.	75	56
80...	60	36	30	42	40	37	45	44	35	90	55	46
90...	36	30	29	35	35	31	40	37	29	61	45	43

The relation of success in these performance tests to chronological age is evident in the percentile tables. We thought it would be interesting to determine whether the relationship to chronological age were closer than the relationship to mental age, as determined by the Stanford Binet test. Accordingly we selected all of the children for whom we had records of mental age and figured a Pearson correlation, first with chronological age and then with mental age. Since only cases in which the children had succeeded in the performance test could be used in figuring a correlation, the number of cases on which a correlation could be based proved to be only from twenty to twenty-five for each test. The number is too small to give a result of high scientific value. It is presented here merely as a suggestion of the probable relationship.

CORRELATIONS—PEARSON COEFFICIENT

	Pink Tower	Cylinder *A*	Cylinder *B*	Cylinder *C*
Mental Age	.19	.02	.21	−.13
Chronological Age	.27	.11	.27	.23

In every case the chronological age gives a higher correlation with the performance test than the mental age. It seems probable that within the limits of normal mental ability represented in this group of children, mere age, connoting length of experience, is more important than mental level in determining ability to make correct judgments of size and shape, and ability to adopt a method of procedure which is capable of carrying a simple task to completion.

Comments.—The behavior of each child during the test was carefully observed, and notes made as to his method, his handling of the blocks and cylinders and his general attitude, as indicated by his manner, the expression of his face, and any comments he made. While the main value of these observations is in their contribution to other studies made of the individual children, a summing up of typical reactions may be worth while.

Method.—In fitting the cylinders into the holes, some children used a definite method from the start, either looking

at the hole first or choosing the cylinders first and proceeding in an orderly way from largest to smallest, or vice versa. Others went at it in a hit or miss fashion, picking up a cylinder at random and trying it in any hole without noticing relative sizes. Some began in a random way and later developed a method. A few used method with the first two sets and abandoned it with the third set. In general the use of method was more characteristic of the older than of the younger children, and was particularly marked in the five-year-old kindergarten children, many of whom were so obsessed by the idea of orderly progression that they would return a cylinder to the bench, although they perceived the proper hole for it, rather than momentarily skip a hole or fail to take the next in order. Some would try persistently to fit cylinders into holes too small, or repeat the same errors; other would grasp the situation at once or learn in one trial.

Handling.—Some children grasped the blocks and cylinders firmly and confidently, others in a fumbling and uncertain manner. Some placed the blocks with precision, others were content with a weak and wobbly structure. The manner of handling, however, seemed to have little or no relation to success or failure.

Attitude.—All but seven of the children showed immediate interest and desire to perform the tests. The exceptions were two on the Nursery School waiting list—one very timid, the other an antagonistic mood—two little Jewish boys (twins) at the Balch School, and three Polish children at the Sophie Wright Settlement Day Nursery. These three seemed frightened and bewildered, cried at sight of the stranger, and would not look at the blocks until the enthusiasm of a little Jewish girl reassured them. The contrasts in the attitude of the children at the three day nurseries was interesting. Those at the Tau Beta House—also for the most part Polish—showed no timidity and were very willing to play with the blocks and cylinders. They are accustomed to visits from the wealthy and influential young women who support the Settlement, and had been trained to come forward, take the visitor's hand, and courtesy. They seemed used to attention

and more interested in smiling and being petted than in the game. These two nurseries are managed by capable, sympathetic women, without, however, any special training for the work. Neighborhood House has a regular public school kindergarten in charge of a trained kindergartner paid by the Board of Education. By special arrangement a few three- and four-year-old children are admitted. These children showed no timidity and no suspicion of an ingratiating attitude, but went at the work in a business-like, impersonal manner, which, in contrast with the attitude of the other groups, suggested that even such tiny children absorb a good deal of the prevailing atmosphere.

The older children in the kindergartens were very much at ease, and the Nursery School babies, who had already made friends with the examiner, were the most cordial and responsive of all. Almost all were confident of their ability to perform the tests. Only six of the whole 150 shook their heads, or said "No" when asked if they thought they could build up the tower, and four of these accomplished perfect performances.

There were, of course, wide individual differences in general bearing and manner, arising from racial characteristics, home environment, and natural disposition. Some were independent and responsible, others must be encouraged at every step. "Is that right?" they would ask; "Does it go there?" and refuse to go on until reassured. Some talked continually, others were silent. One little red-headed Irish boy shouted joyously, as he picked up each cylinder, "Where the hell does this guy go?" A very self-contained little fellow, who hadn't said a word until he came to the last set of cylinders, broke his long silence by responding scornfully to the examiner's cheerful remark that this was harder, "I bet it ain't! I bet I can do it as easy as anything!" and proceeded to prove that he couldn't. One or two demurred about building the tower and desired to build a house instead. Some went at it soberly, some laughed or crowed and jumped with joy at each success. Some dramatized the game, apostrophizing the cylinders with "You go there!" or "Oh,

you!" with annoyance at a wrong fit. A few chanted little songs, such as "This one here, and this one here," or "I know where this goes and I know where this goes." Some concentrated steadily, some were subject to lapses of attention or were moved to engage in irrelevant conversation. Some performed every act with decision, others were given to hesitation. All these differences, of course, stood out more markedly under the uniformity of conditions provided by the tests. It is felt, therefore, that the tests, besides measuring satisfactorily definite abilities, contribute distinctly to studies made of individual children.

AN EXPERIMENTAL STUDY OF THE INFLUENCES OF ORGANIZATION OF MATERIAL FOR MEMORIZING UPON ITS RETENTION

BY DONALD A. LAIRD,[1] HERMAN REMMERS AND LLOYD J. PETERSON

I. The Problem

There are a multitude of factors influencing the strength of associations, the permanency of bonds, or the accessibility of action patterns, depending upon whichever structural type of psychology is preferred. The present communication is concerned with the relative effects of perusal of organized or unorganized materials to be remembered, upon the permanency and amount of the material remembered.

Ebbinghaus, working with nonsense syllables, obtained a much more precipitous curve of forgetting than did Radossawljewitsch who used meaningful materials. Thus, comparing two different groups of observers memorizing entirely different materials, we find the balance retained in favor of the meaningful material. The present study is directed toward the single factor of organization of the materials in their presentation so far as that can be isolated. The same materials were used, once organized, again unorganized. The same observers were used under apparently identical conditions, save for the organization or non-organization of the material.

II. The Materials

The materials to be memorized consisted of four Anglo-Saxon words and their English equivalents, four additions with their sums, and four obscure historical events with their dates. They are modified from Starch's manual.

[1] Responsibility for this study is distributed as follows: Remmers suggested the original study, Peterson performed the detail work in gathering the data as a project in experimental psychology under Laird, who is largely responsible for the present appearance of the study. Laird is continuing the work on organization in memorizing and will be glad to receive suggestions and criticisms of other workers.

Two *Presentation Lists* of such materials were prepared. The one list, with the items grouped under the appropriate heading, was as follows:

Organized Presentation Material

Four Anglo-Saxon words:

nipan—grow dark
modig—resolute
hyse—warrior
hra—corpse

Four Additions:

593 + 739 = 3211
654 + 723 = 3179
874 + 564 = 1438
752 + 595 = 1347

Four Historical Dates:

Peace of Nimeguen—1678
League of Augsburg—1686
Death of Bilderdijk—1836
Louvain University Restored—1817

Another presentation list, containing essentially the same materials, but arranged without rhyme or reason, was prepared. This unorganized list was as follows:

Unorganized Presentation Material

Surrender of Ostend—1604
Zinc—treasure
634 + 536 = 1280
Erasmus born—1467
snude—quickly
396 + 725 = 1121
metod—Creator
Descartes' death—1650
473 + 825 = 2356
basu—purple
735 + 468 = 1203
Death of Anjou—1584

For purposes of testing the retention two *Recall Lists* were prepared for each Presentation List. These recall lists for the Organized Presentation Material are as follows:

Organized Material, Organized Recall

nipan
resolute
hyse
corpse

593 + 739
3179
874 + 564
1347

Peace of Nimeguen
1686
Death of Bilderdijk
1817

Organized Material, Unorganized Recall

modig
2311
Peace of Nimeguen
resolute
645 + 722
1686
hyse
1438
Louvain University restored
corpse
1836
752 + 595

Similar recall lists, as follows, were prepared for the materials presented for memorizing in an unorganized manner

Unorganized Material, Organized Recall

Surrender of Ostend
Erasmus born
1650
1584

634 + 536
1121
473 + 825
1203

zinc
quickly
basu
Creator

Unorganized Material, Unorganized Recall

1604
metod
1250
Descartes' death
treasure
473 + 825
quickly
basu
1203
Death of Anjou
1467
396 + 725

III. Comments on the Materials

In the Presentation Lists it will be noted that there are two incorrect additions. This device was for the purpose of providing a check upon those who did not memorize the additions but depended upon calculating the sum when the recall was secured. In scoring the recall sheets no such responses were found. After the experimental results were all in the subjects were asked if any of the vocabulary or dates was known to them before the experiment; it was new material to all of them.

In each Recall List half of the items in each division are presented for backward rather than forward recall.

IV. The Subjects

The subjects who contributed the data were forty students in a class in advertising. They ranged from college junior to

graduate student, about half of them were juniors, however. They were summer-school students and above the average in seriousness. They coöperated splendidly in the experiment.

V. The Procedure

On June 20th, at the opening of the regular class hour, the students were confronted by a blackboard, the panels of which were covered with window shades. The students were given the following instructions:

"We are going to perform an experiment in memory. We are taking up this experiment before we study memory so that you will be biased as little as possible. This is an experiment, not a contest. The materials which you are to memorize are on the blackboard. Study them in your ordinary fashion; use whatever memory aids you ordinarily use. Work hard, do your best, but do not get panicky for fear your results are to be compared with those of others. This is an experiment, and not a contest."

Then the shade covering the Unorganized Presentation Material was lifted and ten minutes allowed for its study. When the time allowed had passed the shade was pulled down and the regular class work resumed without further reference to the memory experiment.

Five minutes before the close of the period the students were divided into two groups. Group *A*, on the right of the center aisle, faced the front blackboard. Group *B*, on the left of the center aisle, faced the south blackboard. Each group consisted of twenty students. Group *A* was given the Organized Recall List of the Unorganized Material, Group *B* was given the Unorganized Recall List of the same material. The items in each recall list were exposed by the experimenter and an assistant one at a time and simultaneously for each group. Preliminary work indicated that eight seconds from word to word was the best time for the purposes of this experiment to allow on each recall exposure. This kept the subjects on the alert, allowed sufficient time for writing even the longest item, and did not give them time for making corrections in earlier items.

The subjects were instructed not to make any corrections. In case they failed to get a response for any item to draw a line and let it go at that. The margin of the sheet on which they were to write their recall responses could be used for explaining any items, or their omission, *after* the entire list had been exposed serially.

No further mention was made of this experiment until June 24th, when the two groups were again given the same recall lists in the same manner as before. We thus have the records for immediate and delayed recall, backward and forward, organized and unorganized on material that was unorganized.

The Organized Presentation Material was used on July 6th and 10th, the procedure being the same as for the unorganized material.

VI. The Results

The recall sheets of the subjects were scored in great detail. A record, for each type of recall, presentation, and division of subject matter such as dates, words, and sums, was kept. Correct responses, no response at all, a partial response such as "University of ————," a substitution such as "modig" for "hyse," extraneous substitutions of material not in the lists such as "tree" for "hra," partial substitutions such as "University of Ostend," and miscellaneous errors in recall are tabulated in the following data sheets.

Table I

Immediate and Delayed Recall of the Organized Presentation Material, with the Type of Response given in Per Cent.[1]

	Organized Recall		Unorganized Recall	
	Immediate	Delayed	Immediate	Delayed
Correct	51	47	25	25
Substitutions	3	3	5	5
Extraneous substitutions	0	0	1	3
Partial substitutions	13	11	4	2
No responses	25	36	49	50
Miscellaneous errors	6	2	16	12

10

[1] Slide-rule calculations.

TABLE II

IMMEDIATE AND DELAYED RECALL OF THE UNORGANIZED PRESENTATION MATERIAL, WITH THE TYPE OF RESPONSE GIVEN IN PER CENT.[1]

	Organized Recall		Unorganized Recall	
	Immediate	Delayed	Immediate	Delayed
Correct	38	28	29	19
Substitutions	5	4	4	10
Extraneous substitutions	1	2	7	1
Partial substitutions	10	7	9	12
No responses	42	55	50	56
Miscellaneous errors	2	3	1	1

[1] Slide-rule calculations.

Chart 1 presents a comparison of the two Groups in

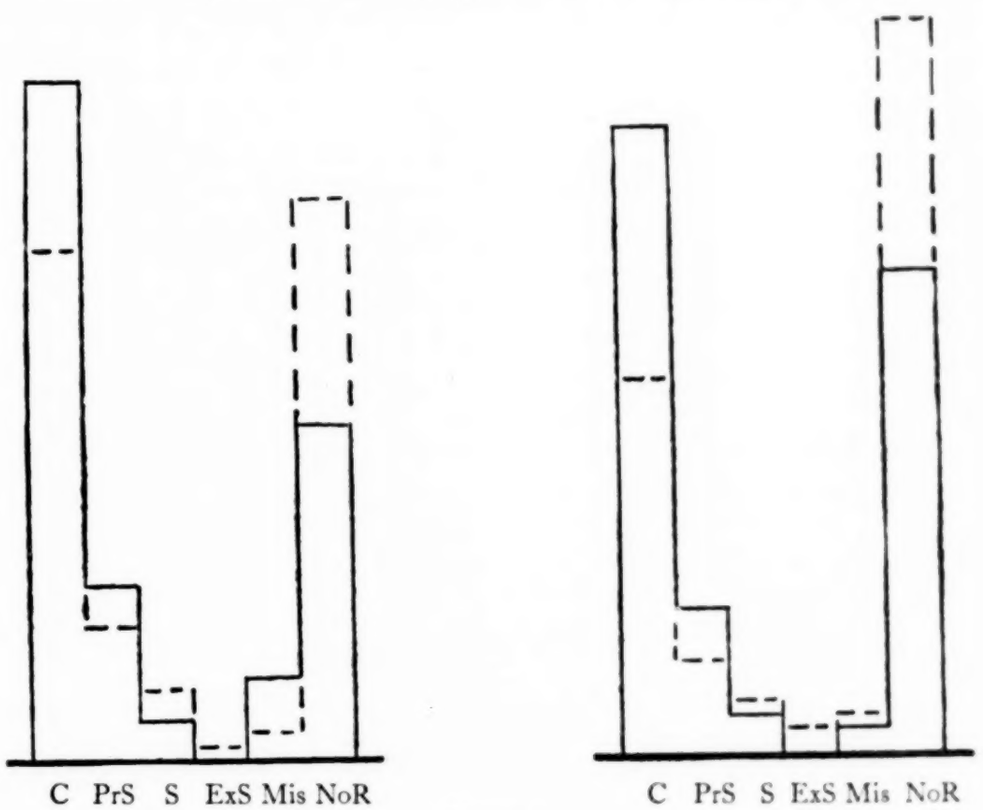


CHART 1

Comparison of effect of organized and unorganized presentation upon organized recall. Solid line—organized material. Broken line—unorganized material. *Y*-axis indicates per cent. of each response.

Immediate recall to left, delayed recall to right.

organized recall. Each figure has the type of responses arranged from correct at the left to no response to the right.

In the type of response between these extremes it has been attempted to arrange them in their relative order. This will enable a fairly direct comparison to be made since increasingly poor responses go toward the right of each figure.

In both immediate and delayed recall of the organized recall lists the organized presentation is found to excell greatly the unorganized. This difference in favor of the organized presentation becomes greater with the delayed recall. Extraneous substitutions are found only in the unorganized presentation, perhaps due to a spontaneous attempt to effect an organization of some sort with already existing memory materials. This 'working over' of the materials presented after the period of immediate recall, however, seems to be greater for the organized material since the memories of this material does not fade nearly so rapidly as is the case with the other material.

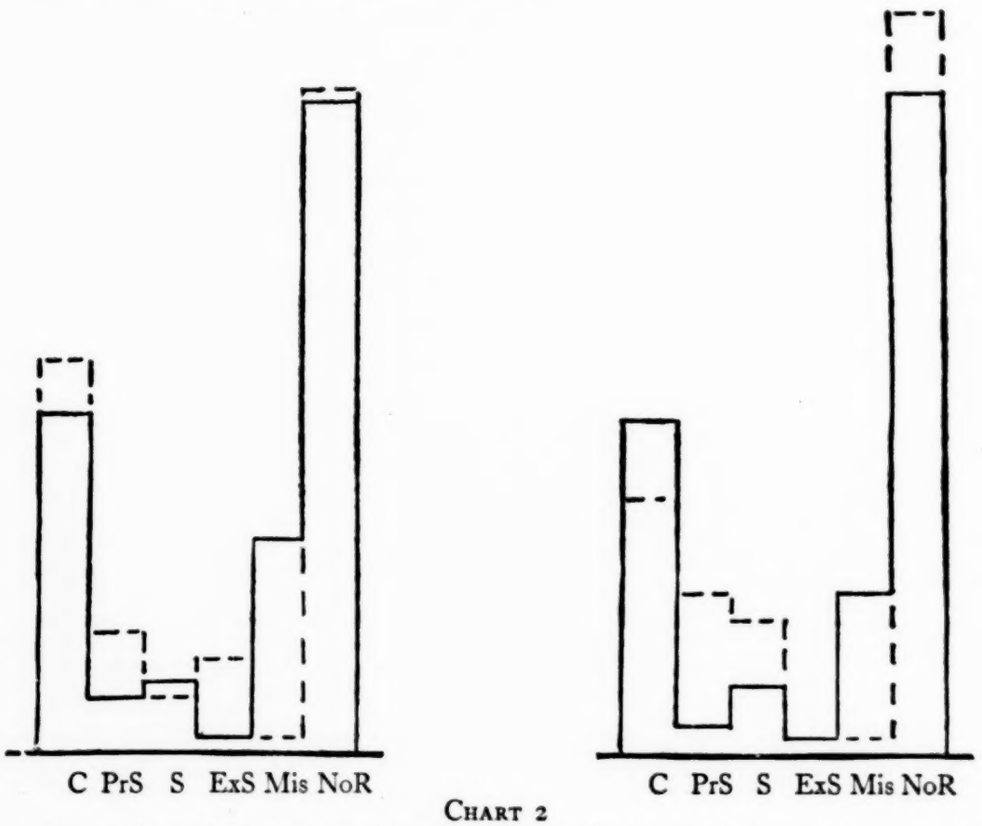


CHART 2

Comparison of effect of organized and unorganized presentation upon unorganized recall. Solid line—organized material. Broken line—unorganized material. *Y*-axis indicates per cent. of each response.

Immediate recall to left, delayed to right.

When we return to the unorganized recall of this same material (Chart 2) we find somewhat different results. In the first place the difference between the two groups of presentation materials for immediate memory goes in a direction opposite to what it did with organized recall. Here we find more correct and better responses with the unorganized materials. In the case of the delayed recall, however, we find the advantage reversed and slightly in favor of the organized presentation, if it is anything more than a draw.

Apparently here we have something analogous to the modern doctrine of formal discipline. When the items are memorized in the arrangement most nearly like the way in which they are to be recalled, the more nearly perfect that recall will be, especially in the case of immediate recall.

This is what we find for the 'general faculty of memory' as evidenced by the composite results from forty individuals on memory for three sorts of facts. Turning now to the effects of organization, or non-organization, upon memory for sums, dates, and words we have the following data:

TABLE III

IMMEDIATE AND DELAYED RECALL OF THE ORGANIZED PRESENTATION MATERIAL, WITH THE TYPE OF RESPONSE GIVEN IN PER CENT. AND BY CLASS HEADS

	Organized Recall						Unorganized Recall					
	Immediate			Delayed			Immediate			Delayed		
	Words	Sums	Dates	Words	Sums	Dates	Words	Sums	Dates	Words	Sums	Dates
Correct	62	26	65	58	30	54	40	13	23	38	11	25
Substitutions	6	5	0	7	2	2	15	1	0	6	1	6
Extraneous substitutions	0	0	0	0	0	0	4	0	0	6	0	3
Partial substitutions	18	12	9	13	9	10	3	6	3	0	3	4
No responses	10	49	18	21	60	28	30	75	41	46	79	35
Miscellaneous errors	5	9	10	2	2	6	9	5	34	4	6	31

In order to make possible a ready comparison of the influences of organization in presentation upon the retention of the various classes of material the losses and gains were

TABLE IV

IMMEDIATE AND DELAYED RECALL OF THE UNORGANIZED PRESENTATION MATERIAL, WITH THE TYPE OF RESPONSE GIVEN IN PER CENT. AND BY CLASS HEADS

	Organized Recall						Unorganized Recall					
	Immediate			Delayed			Immediate			Delayed		
	Words	Sums	Dates	Words	Sums	Dates	Words	Sums	Dates	Words	Sums	Dates
Correct	50	22	41	32	18	32	50	13	23	33	4	19
Substitutions	10	2	3	12	0	0	10	0	0	9	9	11
Extraneous substitutions	2	0	2	6	0	0	0	8	12	1	1	0
Partial substitutions	2	18	10	0	13	9	0	14	12	4	17	16
No responses	36	57	36	47	62	56	40	64	50	50	67	51
Miscellaneous errors	0	3	9	4	8	4	1	0	4	2	1	2

computed and are given in Table V. The column to the left gives in italics the type of recall and the six sub-headings under this indicate the responses made. The four columns to the right of this give the differences in per cent. of each form of response in the case of organized material versus unorganized material. When the correct responses in the organized material exceeded those of the unorganized material the difference is inserted to the left side of the row; differences due to an excess in the responses from the unorganized presentation material are inserted to the right behind a minus sign.

Fewest correct or partially correct responses were made in the sums (Table IV.). Under the conditions of study it is apparent that the sums were the most difficult to remember. From Table V. we find that in organized recall the advantage is to the organized presentation, especially in the delayed recall. In the unorganized recall of the sums the advantage is shifted to the unorganized presentation, and again greatest in the delayed recall.

The most marked benefit from organized presentation is found in the dates, but here the immediate advantage is greater than the delayed. The unorganized recall is facilitated by the unorganized presentation.

The vocabulary is midway between the sums and the dates in the benefit derived from organization, although

TABLE V

COMPARISON OF INFLUENCES OF ORGANIZATION UPON MEMORY FOR WORDS, SUMS, AND DATES AND THEIR COMPOSITE [1]

	Composite	Words	Sums	Dates
Organized Recall—Immediate				
Correct	13	12	8	24
Partial substitutions	3	16	− 6	− 1
Substitutions	− 2	− 4	3	− 3
Extraneous substitutions	− 1	− 2	0 0	− 2
Miscellaneous errors	4	5	6	1
No response	−17	−26	− 8	−18
Organized Recall—Delayed				
Correct	19	16	12	22
Partial substitutions	4	13	− 4	1
Substitutions	− 1	− 5	2	2
Extraneous substitutions	− 2	− 6	0 0	0 0
Miscellaneous errors	− 1	− 2	− 6	2
No response	−19	−26	− 2	−28
Unorganized Recall—Immediate				
Correct	− 4	10	0 0	0 0
Partial substitutions	− 5	3	− 8	− 9
Substitutions	1	5	1	0 0
Extraneous substitutions	− 6	4	− 8	−12
Miscellaneous errors	15	8	5	30
No response	− 1	−10	11	− 9
Unorganized Recall—Delayed				
Correct	6	5	7	6
Partial substitutions	−10	− 4	−14	−12
Substitutions	− 5	− 3	− 8	− 5
Extraneous substitutions	2	5	− 1	3
Miscellaneous errors	11	2	5	29
No response	− 6	− 4	12	−16

[1] See text for instructions for reading this table.

somewhat closer to the dates. In the organized recall but slight advantage over the immediate is given the delayed recall by organized presentation. In the unorganized recall, however, the immediate benefits vastly more than does the delayed from presentation and study in an organized manner. This one instance is the only consistent exception to the generalization given above regarding unorganization benefiting unorganization.

The relation between meaningful material and organization

is fairly apparent. The sums were without doubt the least meaningful, the vocabulary the most meaningful. The advantage from organized presentation is greatest for the more meaningful materials. This shows up plainly in the organized recall and also in the unorganized recall. The great advantage of the organized presentation in the unorganized recall of the words may be accounted for in this way.

VII. Summary

Concerning the experiment as a whole, it must be said, of course, that the cases are too few to warrant any far-reaching deductions. Psychologists, par excellence, and when working in the field of learning especially, have been far from free from what Angell has termed the 'incurable tendency to generalize.' The learning of nonsense syllables has furnished a basis for much questionable generalization anent the learning process. All the available evidence points to the conclusion that learning is *specific*, and not general. In view of this, it becomes necessary to suspend judgment on the variation of recall with the variations in presentation for any material except such as concerning which there are adequate experimental data available. This means that the method of learning geography must be determined experimentally with geographical material, and not with Anglo-Saxon words or sums, or card-sorting or nonsense syllables. The variations between vocabulary, sums, and dates in the present study bear this out.

Further experimentation of the kind here reported should be carried out in order to determine with fair statistical accuracy measures of differences in results.

This study seems to warrant the following conclusions:

1. Materials to be learned should be classified in some orderly or logical fashion. Even in the case of unorganized recall this organization in presentation works out in the long run to the benefit of the retention.[1]

[1] This is corroborated by an unpublished study by Remmers, in which the two lists of materials used for the present experiment were given on days widely separated to a group of 47 college juniors to memorize with instructions to record the time required to memorize. An average saving of 20 per cent. of time required for learning was effected by organization.

2. On the whole it seems probable that increasing benefit is derived from organized presentation with increasing meaningfulness of the material under examination.

3. The balance in favor of organized presentation becomes greater with delayed recall.

4. That the mental life spontaneously attempts some sort of an organization is evidenced by the finding of extraneous substitutions only in the unorganized presentation material.

5. The benefits derived from organization are lessened materially with unorganized recall.

6. The composite findings—pertinent, perhaps to the 'general faculty of memory'—do not agree except in a loose way with the findings for the three types of material used.

A NOTE ON THE CONDITIONED EYELID REACTION

BY HULSEY CASON
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In a paper published in the JOURNAL OF EXPERIMENTAL PSYCHOLOGY (Vol. 5, 1922, pp. 153–196) on "The Conditioned Eyelid Reaction," I failed to state clearly just what comparisons could be made in regard to the absolute time of the eyelid reactions which I obtained. In this experiment, the subject was made to wink several hundred times by the electrical stimulation method, and simultaneously with this wink a sound stimulus was given. After this 'training' had been continued for some time, winks could be evoked by giving the sound stimulus alone, and the time of these conditioned wink reactions was somewhat faster than the subject was able to wink *voluntarily* to the same sound stimulus. The sound stimulus alone was not strong enough to cause a reflex wink at the beginning of the experiment. The main comparison which I wished to make was a *relative* one between the conditioned reaction time and the voluntary reaction time. Some psychologists may be interested in comparing my wink times with the fastest voluntary reaction times of the better known reaction time experiments. It may also be desirable to compare the wink times I obtained with those obtained by Dodge and others. I therefore add some further data which will make such comparisons more accurate.

The wink times I give in the experiment referred to above were not measured from the sound stimulus to the *beginning* of the winking movement. The wink times include not only the time which elapsed between the sound stimulus and the *initiation* of the movement, but also the time which elapsed while the eyelid was closing part of the way. Otto Weiss[1] gives the average closure time as 60σ, ranging from 43σ to

[1] 'Die zeitliche Dauer des Lidschlages,' *Ztschr. f. Psychol. u. Physiol. d. Sinnesorg.*, 1911, 45, 1, Abt., 307–312.

107σ. Dodge's curves for the wink reaction show the closure time to be somewhat less.[1] This time naturally varies for different subjects and in the same subject at different times, and a very accurate comparison cannot be made. With the apparatus I used, the subject's eyelid moved about 5 mm. before the electrical contact was made, and before the chronoscope was stopped. It is reasonable to deduct some 40σ (because of the time lost while the eyelid was moving the 5 mm.), if a comparison of the absolute wink times is to be made. The latent time of the inductorium (used to call out reflex winks) was 10σ, and the latent time of the relay or telegraph sounder (used in giving the conditioning stimuli) was 12σ. A further deduction might be made for the time lost while the sound was traveling through the air. For most subjects this amounted to about 4σ. If absolute time comparisons are to be made, some 50σ should be deducted from the wink times as given in my paper. Any slight disparity which may still exist between my figures and those given by Dodge and others can be accounted for by a difference in the *intensity* of the sound stimuli used, my sound stimuli being of comparatively *low* intensity.

In this experiment, the conditioned eyelid reactions obtained were not 'voluntary reactions.' I gave no 'ready' signals, which decrease the time in the usual reaction time experiments. A voluntary wink reaction is definitely slower than a reaction made by raising the finger from a telegraph key. Reacting voluntarily to sound by raising the finger from a telegraph key is rarely less than 110σ; and in many cases my conditioned wink times were considerably less than 90σ. I feel justified therefore in concluding that most of the conditioned eyelid reactions which I obtained have some of the characteristics of a 'reflex.' 'Voluntary factors' certainly cannot account for the effect produced.

[1] 'A systematic exploration of a normal knee jerk,' *Zeitschrift für allgemeine Physiologie*, 1911, 12, 1–58.